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HALLUCINATIONS PRODUCED BY SENSORY CONDITIONING *

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INTRODUCTION

The literature of 'psychic' phenomena is replete with incidents in which perceptions are reported to have occurred in the absence of physical events appropriate to evoke them. In many cases the event responsible for the perception has been demonstrated; in other cases this has not been done. In many of these instances psychologists and others have described the phenomena as hallucinations similar to those experienced by the mentally ill and by normal persons under the influence of drugs. However, in spite of the importance of this topic both for psychical research and for psychology itself, comparatively little work has been done on the nature of hallucinations in normal persons uninfluenced by drugs. The present experiment was designed to study that problem, using a modified conditioned response technique.

Perceptions occurring in the absence of adequate stimuli have been studied under two headings: suggestion and sensory conditioning. The majority of the techniques used in the suggestion experiments designed to produce hallucinations are represented in the extensive works of Warner Brown (**3**) and Seashore (**16**).[1] The general procedure used in these sensory-suggestion experiments was to show the subject an elaborate apparatus for the production and control of a simple stimulus such as warmth, odor, or electric shock. The fact that the apparatus could produce the stimulus and increase it very gradually was demonstrated to the subject. Then in a mock sensory acuity test the stimulus was omitted, although the experimenter pre-

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[1] A bibliography of suggestion experiments designed to produce hallucinations may be found under the topic "Illusions," in "Suggestion and Suggestibility: A Bibliography," by C. BIRD, in the *Psychological Bulletin*, 1939, **4**, p. 277.

tended to produce it. Under these conditions Brown found that 60 percent to 90 percent of his subjects reported a perception in the absence of the stimulus. Similar results were obtained by suggesting a change in a stimulus such as an increase or decrease in brightness, pitch, and size.

In hallucinatory sensory conditioning one attempts to produce similar false perceptions by the repeated pairing of one stimulus with another, rather than by suggestive instructions and elaborate apparatus. Sensory conditioning data have been obtained from three sources: (*a*) incidental reports from subjects in non-sensory conditioning experiments, (*b*) conditioning under hypnosis, and (*c*) experiments specifically designed to study sensory conditioning.

(*a*) *Incidental Reports from Subjects in Non-Sensory Conditioning Experiments*

Garvey (**8**), who by means of a shock as the unconditioned stimulus conditioned respiratory changes to a vibrator and buzzer separately and then tested the stimuli separately and together, states that 6 of 8 subjects reported shock with the double stimulus, although actually this combination was never given. One subject reported each day for 3 days that the shock came with the double signal usually, and seldom in any other combination. This suggests that for this subject the shock was below the threshold of report during conditioning.

A second case of accidental sensory conditioning is found in a paper by L. E. Cole (**5**). In the report of a conditioned eyelid experiment he gives the following note (**5**, p. 360, footnote 3): "At the 13th non-reinforced trial the subject reports, 'That shock was awfully weak.'" The writer, in a conditioned galvanic skin response experiment (**6**) heard similar incidental reports from subjects also indicating the occurrence of sensory conditioning. Such items suggest that sensory phenomena can be conditioned, although experiments specifically designed to obtain sensory conditioning have not met with a corresponding success.

Mowrer (**13**, p. 82), in an experiment investigating preparatory set, used a shock which built up gradually from zero to a suprathreshold value at regular intervals. The subjects reported when they felt the shock. Mowrer states, "If, after a series of presentations of the gradually increasing shock, the shock was completely withheld for a time, subjects almost invariably pushed the key . . . indicating an hallucinatory perception of the shock." As Mowrer suggests, this phenomenon may be similar to the anticipatory and perseverative effects which complicate the determination of absolute sensory thresholds.

(*b*) *Sensory Conditioning Under Hypnosis*

Rather striking evidence of hallucinations in normal subjects has been obtained in experiments combining hypnosis and conditioning. Scott (**15**) conditioned finger withdrawal from shock to the sound of a buzzer. Half of his subjects were conditioned during hypnotic trance and tested after awakening; the other half, under the reverse conditions. In test trials, subjects in both groups reported pain, tingling, or shock upon presentation of the buzzer. One subject jerked his hand back and reported shock when being adjusted into the apparatus for a waking retest. Leuba (**10**) obtained similar results in a series of informal experiments in which two stimuli were paired while the subject was under hypnosis. When one of the stimuli was presented alone after the subject had been awakened, 14 of 16 subjects tested gave some behavioral or verbal indication that they perceived the second stimulus. However, these experiments in which the subject reports an hallucinatory sensation after awakening from the hypnotic trance are subject to certain criticisms. An obvious one arises from the fact that the evidence is behavioral as well as verbal. Because of the hypnotized subject's well-known propensities to rationalize peculiar behavior resulting from post-hypnotic suggestion, it is to be expected that a subject who jumped at touching a dead electrode or scratched his arm would say that he did this because he received a shock or felt an itch. Ordinary motor conditioning might thus easily have consequences which could be interpreted on a sensory basis.

The applicability of this criticism of the experiments using hypnosis is of course dependent upon the definition of an hallucination. An hallucination must be defined in terms of a verbal report, *i.e.*, as a perception reported in the absence of an adequate stimulus. To make the above criticism permissible in the face of this definition, a qualification must be made to the effect that the report of the perception must be given under conditions in which falsification would be unlikely. This condition is probably not satisfied during a test period immediately following the hypnotic trance because of the above-mentioned tendency toward rationalization.

(*c*) *Experiments Specifically Designed to Study Sensory Conditioning (Excluding Hypnosis)*

An experiment by E. L. Kelley (**9**), who attempted to produce hallucinations (chromaesthesia) by a conditioning procedure in unhypnotized subjects, obtained completely negative results. He paired tones with colors, with one of his subjects as many as 3000 times, but no subject reported a color when the tone was presented alone.

With the exception of the studies by Brown (3) and Seashore (16), the experiments summarized up to this point have been concerned with hallucinations produced by conditioning. Those which follow are included to complete the bibliography of sensory conditioning papers although they do not involve hallucinatory conditioning. They are of interest in suggesting the conditions under which sensory phenomena may be conditioned.

Cason (4), the first to use the term 'sensory conditioning,' obtained negative results in an attempt to condition a sensory summation effect. Finding that the sounding of a bell raised the apparent intensity of a light, and vice versa, he attempted to exaggerate this effect by repeated pairing of the two stimuli. All of his four slightly differing procedures resulted in a decrease in the summation effect rather than the increase to be expected if conditioning had occurred.

Only two experiments specifically designed to study sensory conditioning without hypnosis have given positive results. Bogoslovski (1) demonstrated conditioning, extinction, and more rapid reconditioning of a change in the electrical sensitivity of the dark-adapted eye. He used a light as the unconditioned stimulus, a time interval and the sound of a metronome as conditioned stimuli. The conditioned response disappeared when the subject was told not to expect the light, or could be evoked by having the subject imagine the presence of the light. Unfortunately, Bogoslovski does not present details concerning the conditions under which the electrical threshold was determined, except in other papers not available to the writer. These conditions are rather crucial to the interpretation, and without them and without similar positive results in other experimental situations we cannot conclude with Bogoslovski that "There can be hardly any doubt that the rule observed by us for the electrical sensitivity of the eye must be valid for all the sensorial sphere"!

The second experiment obtaining positive results was that of von Fieandt (7) who succeeded in conditioning the brightness constancy phenomenon to both visual and auditory stimuli. In a situation arranged so that a stimulus area appeared either as light gray in shadow or as dark gray in no shadow, he was able to control the perception according to the presence or absence of a black button in the visual field or a bell signal. Forty to 80 pairings of these stimuli were required.

Sensory conditioning of the hallucinatory type might possibly be inferred from an experiment with dogs performed by Brogden (2). After pairing a bell and a light for 200 trials, he conditioned a flexion response to one of these stimuli, and found that it immediately transferred to the other. However, a parallel experiment by Lumsdaine (11) on human subjects indicates that an explanation in terms of

sensory hallucination is questionable, or at least not necessary. Lumsdaine (**11**), using a technique previously developed by Shipley (**17**), paired a light with a tap below the eyelid, then conditioned a finger retraction to the tap. The light alone then evoked the finger retraction. These results were interpreted in terms of chain conditioning, mediated by the proprioceptive stimuli from the conditioned lid reaction to the light. An alternative interpretation in terms of a central 'set' was also given since the lid reaction sometimes coincided with or even followed the finger response. Although no mediating motor responses corresponding to the lid reaction are known to occur to the stimuli used by Brogden, their presence is not excluded from consideration.

No certain conclusions may be drawn from the foregoing evidence as to the conditions under which sensory conditioning does or does not occur. However, several possibly favorable conditions are suggested: (*a*) an emotion-arousing unconditioned stimulus such as electric shock (**5, 8, 13**); (*b*) dissociation of the pairing and testing situation (**10, 15**); (*c*) a subliminal unconditioned stimulus or one not attended to (**8**); and (*d*) difficulty of discrimination between the presence or absence of the objective stimulus (**13**, possibly **8**). Also, on theoretical grounds one might expect (*e*) motivation, a desire to perceive, to be a fifth favorable factor.

The procedure used in the present study was evolved after considerable preliminary experimentation with factors *a*, *c*, *d*, and *e*. Promising results were finally obtained with a situation involving factors *d* and *e*. A light and a tone were paired in what was essentially a delayed conditioned response situation. The onset and termination of the unconditioned stimulus, the tone, were gradual. This condition presumably makes an intensity discrimination between an hallucination and the stimulus tone extremely difficult or impossible. Motivation was introduced by means of the instructions which described the experiment as an acuity test.

Because the conditioning situation resembled that used in the earlier suggestion experiments, controls were run to determine the amount of suggestion present in the instructions and in other parts of the procedure.

Apparatus and Subjects

The subject and experimenter were in adjoining rooms, the subject in a Morris chair with his back to a window. During experimentation, the door between the two rooms was closed. There was no acoustic control except that the experiment was interrupted during the occasional auditory presence of a noisy delivery truck, passing airplanes, and the campus chimes which struck at 15-minute intervals. The noise level may be characterized as that of a third floor room opening upon a relatively quiet part of the campus.

The unconditioned stimulus was a 1000 cycle tone produced by a General Radio B.F. Oscillator, the intensity being controlled by an attenuation box calibrated in 2-decibel units. The

tone was delivered through a loudspeaker located in the experimenter's room about 10 feet from the subject's chair and was extremely difficult to localize. The conditioned stimulus was the lighting of a clear-glass 2½ volt bulb placed on the left arm of the chair. A 2 × 3 inch rectangle of white cardboard behind the light served as a reflector or background. On the right arm of the chair a telegraph key was placed, operating a combined light and click signal in the experimenter's room. The experimenter's table carried an electric clock and a timer which gave faint clicks (inaudible to the subject) at 1-second intervals.

The subjects were men and women students from the elementary psychology class, mostly sophomores. They took part in this and other experiments as a part of the course requirements. The distribution of the sexes among the experimental groups was as follows: Group I (experimental group, 23 men and 17 women; Group II (control), 52 men and 38 women (this group was formed of three sub-groups, each containing 30 subjects divided as follows: Group II-A, 18 men and 12 women; Groups II-B and II-C, each 17 men and 13 women); and Group III (control), 39 men and 21 women.

Procedure and Results

The procedure was roughly that of a motor conditioning experiment. The light (CS) was paired with the tone (UCS), the subject being instructed to press the key when he heard the tone. The tone differed from the unconditioned stimuli used in conventional conditioning experiments in that its onset and decline were gradual, coming above threshold usually several seconds after the onset of the light. In training trials the (subthreshold) onset of the tone coincided with the onset of the light. In reinforced test trials an interval of 15 or 30 seconds elapsed between the onsets of the light and tone. Consequently, if the subject pressed the key during the period when the light was on but the tone was not, a conditioned or hallucinatory response was recorded. In the main experiment, with one exception (Group III), all test trials were reinforced with a tone, either immediately following the pressing of the key or, if no response occurred, after the test interval.

I. Experimental Group

Group I (40 subjects)

The procedure and results for Group I, the experimental group, will be described first. This group was given 60 conditioning or training trials, with test trials at intervals during training, and also following training. When the subject came to the experiment, he was told to be seated in the Morris chair. The tone was turned on at an intensity about 20 db above threshold, and left on while the instructions were given. The instructions, designed to give as little suggestion as was compatible with telling the subject what to do, were as follows:

> "The tone which I have just turned on is the one I am using in this experiment. I want to determine how faint a tone you can hear. What I want you to do is to press that key as soon as you hear the tone, hold it down as long as you hear it, and let it up when you don't hear it any longer. Please pay very close attention because the tone is very faint most of the time. The only other thing you have to do is to keep your eyes open so that you can see that light, which is a ready signal. You don't have to watch it; just be sure you can see it when it comes on."

Questions from a few subjects were answered by repetition of parts of the instructions if possible, otherwise as little information as possible was given. Questions concerning the purpose of the experiment were answered by a restatement of the misinforming 'Call for Subjects,' which had been circulated in the classroom, wherein the subjects were told that the purpose of the study was to measure auditory thresholds.

The subject was then left alone and given 60 training trials. In these the light and tone were turned on simultaneously, the light then remaining on (as also in all reinforced test trials) until the subject released the key signifying that he no longer heard the tone. The tone began at an intensity at which no subject reported hearing it at the beginning of the experiment, and was increased 2 db/second until the subject pressed the key. It was maintained at that level for two seconds and then decreased 2 db/second until the subject released the key, when both light and tone were turned off.

For the first five trials the tone was initiated at the same low intensity to determine a tentative threshold. On the 6th trial the tone began at an intensity 4 db below the lowest intensity at which the subject had responded in the first 5 trials. Thereafter the initiating intensity of the tone was controlled so as to keep the interval between the onset of the light and the pressing of the key by the subject roughly between 2 and 6 seconds. This was done by raising or lowering the initial intensity of the tone by 2 db if the interval was greater than 6 seconds or less than 2 seconds in three of any seven consecutive trials. This procedure served to maintain the interval between the onset of the CS and the onset of the UCS within the limits stated, in the majority of the trials. It is evident from the above that in the present experiment the interval between the onset of CS and the UCS and the duration of the UCS were not fixed as in the usual conditioning experiment. The subjective onset and the cessation of the tone depended upon a fluctuating threshold with the result that the conditioning interval and the duration of the UCS must be described by frequency distributions.

The mean interval between the onset of the light and that of the tone for all subjects in this group was 3.6 seconds, the corresponding mean for the duration of the UCS being 5.0 seconds. Percentage frequency distributions for the obtained values of the conditioning interval and for the duration of the UCS, together with corresponding distributions of subject means, are presented in Table 1.

TABLE 1

PERCENTAGE DISTRIBUTIONS OF (*a*) THE INTERVALS BETWEEN THE ONSET OF THE LIGHT (CS) AND THE SUBJECTIVE ONSET OF THE TONE (UCS), AND (*b*) THE SUBJECTIVE DURATION OF THE TONE (UCS), TOGETHER WITH THE CORRESPONDING DISTRIBUTIONS OF THE SUBJECT MEANS

The figures in the second and third columns represent the percentage of the total cases and subject means respectively in each interval-category shown in the first column.

(*a*) CS–UCS Interval			(*b*) Duration of UCS		
Length of Interval (Seconds)	Percentage of Obtained Values	Percentage of Subject Means	Length of Interval (Seconds)	Percentage of Obtained Values	Percentage of Subject Means
0– 0.99	1.8				
1– 1.99	10.6		0– 1.99	2.3	
2– 2.99	20.1	17.5	2– 2.99	6.2	
3– 3.99	23.5	67.5	3– 3.99	18.3	32.5
4– 4.99	18.6	12.5	4– 4.99	23.5	30.0
5– 5.99	12.0	2.5	5– 5.99	18.8	10.0
6– 6.99	5.0		6– 6.99	12.7	17.5
7– 7.99	3.5		7– 7.99	7.7	2.5
8– 8.99	1.7		8– 8.99	3.6	7.5
9– 9.99	1.1		9– 9.99	2.0	
10–10.99	0.6		10–10.99	1.6	
11–17.99	1.5		11–22.99	3.4	
Total %	100.0	100.0	Total %	100.1	100.0
N	2400	40	N	2400	40

The intervals between the end of one trial and the beginning of the next were from 15 to 20 seconds. After the 20th, 40th and 60th training trials, three reinforced test trials were given in which the light was turned on 15 seconds before the tone. Following the last (9th) 15-second test trial, a series of ten 30-second reinforced test trials were given. This series of 10 test trials is the basis of comparison with a corresponding series of 10 tests given to the control groups. In these reinforced test trials the onset of the light preceded that of the tone by 30 seconds (if the subject did not press the key in the interval). At the end of the interval the tone was turned

on and its intensity increased until the response occurred, and then was gradually decreased as in the training trials. If the subject pressed the key in the interval, the hallucinatory hearing of the tone was reinforced by turning on the tone, increasing its intensity very rapidly to a value estimated to be slightly above threshold, then decreasing it gradually until the subject released the key.

Following the 10 test trials the subjects were given a 'rest period,' During this they were asked in a conversational manner, "Are you having any difficulty telling when to press the key?" The answer to this was followed by "Do you ordinarily press the key as soon as you hear the tone or do you wait until you are sure?" The order of the alternatives was reversed with about half of the subjects to balance the effects of suggestion. This terminated the experiment proper for Group I.

All but three of the subjects said that they waited until they were sure of hearing the tone before pressing the key. Consequently, to determine the effect of a criterion of hearing the tone that differed from the one spontaneously adopted by most of the subjects, new instructions were given to 30 of the 40 subjects in this group. These were followed by a second set of ten reinforced tests. The new instructions (called 'not-sure instructions') are given below:

> "From now on press the key just as soon as you hear the tone. Do not wait until you are sure, but press the key immediately, at the same time trying not to respond to outside noises which are not the tone."

The subjects were then given 10 additional 30-second reinforced test trials under the new instructions, followed by an 'extinction series' consisting of twenty 60-second *unreinforced* test trials. In the latter the tone was not presented at all. The light was turned on and remained on for 60 seconds if no response occurred. If the subject pressed the key during this interval the light was turned off as soon as the key was released.

To prevent the subjects from discovering the true nature of the experiment, extinction was discontinued for those who gave no response on the first five unreinforced tests. They were given a few training trials and dismissed. This avoided a long series of trials in which no tones were heard.

A summary of the steps of procedure for Group I follows:

I. 20 training trials (1–20)
II. 3 15-second reinforced tests (1–3)
III. 20 training trials (21–40)
IV. 3 15-second reinforced tests (4–6)
V. 20 training trials (41–60)
VI. 3 15-second reinforced tests (7–9)
VII. 10 30-second reinforced tests
VIII. 'Rest period,' questions, and 'not-sure instructions'
IX. 10 30-second reinforced tests
X. 20 60-second unreinforced tests (Extinction series)

Steps II, IV, and VI, which include test trials given at successive stages of training, give data on the acquisition of the conditioned hallucinatory response. The number of subjects giving one or more responses in each of these sets of three test trials is 16, 20, and 18, respectively. (N of the group is 40.) These data have more meaning when supplemented by data from a control group (II-A) which was given test trials under similar conditions but with no preceding training. In the first three test trials given to this group, 4 subjects gave one or more responses with a latency of 15 seconds or less (com-

parable to the 15-second tests given during training) and 7 subjects gave one or more responses in the full 30-second intervals actually used. Since the N of this group was 30, these data must be corrected (multiplied by 4/3) to be comparable to the Group I data. When so corrected the number of subjects giving one or more responses in three tests after no training and after 20, 40, and 60 training trials is 5.3, 16, 20, and 18, respectively. The number of subjects giving hallucinatory responses thus increased with training, the maximum being reached after 40 training trials. However, it should be noted that this is not evidence for a progressive acquisition of the response *by any one subject*, and this evidence is not found when records for individual subjects are examined.

TABLE 2

ORIGINAL DATA FOR GROUP I

Numbers in the body of the table are latencies of hallucinatory responses in seconds for each test trial in which a response occurred. The absence of a latency figure indicates that no hallucinatory response occurred in that trial. Only those subjects who gave one or more responses are represented.

Subject No.	Sex	Test Trial No.										Mean Latency	No. Responses
		1	2	3	4	5	6	7	8	9	10		
1	M	26	—	11	—	16	—	—	—	20	10	16.6	5
2	M	4	—	18	20	24	—	—	—	—	—	16.5	4
3	M	7	8	4	8	8	8	19	18	8	3	9.1	10
4	F	—	—	—	—	—	—	—	—	20	—	20.0	1
7	F	—	—	—	11	12	—	—	12	25	—	15.0	4
8	F	5	7	9	4	7	9	17	4	5	4	7.1	10
9	M	4	3	7	5	4	10	5	26	—	9	8.1	9
10	F	—	—	—	—	28	5	17	—	24	—	18.5	4
11	M	13	15	12	—	3	10	2	19	—	—	10.4	7
12	M	—	—	—	—	—	—	26	—	—	—	26.0	1
13	M	15	—	27	—	—	—	—	—	—	29	23.7	3
14	M	22	—	—	—	24	21	—	13	—	—	20.0	4
15	M	7	—	—	—	—	20	—	—	10	—	12.3	3
16	M	15	—	22	—	—	—	—	12	11	9	13.8	5
19	F	—	—	25	27	—	—	—	—	—	—	26.0	2
22	F	8	12	9	13	—	21	8	22	11	21	13.9	9
23	F	1	6	21	4	8	14	12	13	13	11	10.3	10
24	M	—	—	—	—	26	—	—	—	—	—	26.0	1
25	M	—	—	—	—	—	—	21	17	—	27	21.7	3
26	F	2	—	6	—	4	12	8	10	3	6	6.4	8
27	F	—	—	—	—	3	—	3	—	—	—	3.0	2
28	F	—	—	16	—	—	17	—	—	—	—	16.5	2
29	F	8	—	—	—	11	19	29	8	—	19	15.7	6
31	F	4	6	8	2	8	8	12	9	10	12	7.9	10
32	M	—	7	12	15	—	—	24	18	15	13	14.9	7
33	M	7	18	20	—	—	28	5	—	22	—	16.7	6
34	F	—	—	—	—	25	—	—	—	—	—	25.0	1
35	M	—	12	—	3	—	—	11	—	—	6	8.0	4
36	F	22	—	—	—	10	—	—	—	10	7	12.3	4
38	F	8	8	3	7	5	10	13	24	11	2	10.1	10
39	M	9	14	22	15	13	12	2	1	—	6	10.4	9
40	M	—	—	—	—	—	—	—	12	—	—	12.0	1
Total responses		19	12	18	13	19	16	18	17	16	17		

The ten 30-second test trials (step VII) are the basis of comparison for the experimental and control groups. The original data for those subjects in Group I giving one or more hallucinatory responses in these 10 tests is presented in Table 2. Thirty-two, or 80 percent of the subjects, gave one or more hallucinatory responses.

II. Control Groups

To demonstrate that these results represent sensory conditioning, it was necessary to run a control in which similar test trials were given without the previous training. Group II includes 90 subjects who were given either no training or 10 training trials preceding ten 30-second reinforced test trials similar to those given to Group I. It is composed of three sub-groups, which are combined into the larger group to serve as a single control since they showed no reliable inter-sub-group differences (critical ratios of differences all less than .5, see Table 7). The procedures for the three sub-groups of Group II were as follows:

Group II-A. (30 subjects)

The subjects in this group were given the same instructions as Group I, followed immediately by ten 30-second reinforced tests. Following this they were given the 'rest period,' the questions, the not-sure instructions, and then an additional ten 30-second reinforced tests similar to those given to Group I.

Group II-B. (30 subjects)

This group was given 10 training trials followed by ten 30-second reinforced tests.

Group II-C. (30 subjects)

The procedure for this group was the same as that for Group II-B except that 10 unreinforced tests preceded the training trials. The unreinforced tests were followed by 10 training trials, and ten 30-second reinforced test trials.

The results of the ten 30-second reinforced tests given to each of these three sub-groups were combined as Group II, for comparison with the experimental Group I. Forty-eight, or 53 percent of the 90 subjects in this group, gave one or more hallucinatory responses. The raw data, presented separately for each of the sub-groups, are given in Table 3. Since the chief difference between Group II and Group I is the 60 training trials given to Group I, the difference in proportions of subjects responding may be attributed to the effect of this training.

TABLE 3

ORIGINAL DATA FOR CONTROL GROUP II, PRESENTED SEPARATELY FOR EACH SUB-GROUP

Numbers in the body of the table show latencies of hallucinatory responses in seconds for each test trial in which a response occurred. The absence of a latency figure indicates that no response occurred in that test trial. Only those subjects are represented who gave one or more responses.

Group II-A

Subject No.	Sex	Test Trial No.										Mean Latency	No. Responses
		1	2	3	4	5	6	7	8	9	10		
1	M	—	16	1	7	8	—	—	—	—	—	8.0	4
2	F	—	—	—	—	10	—	—	8	—	14	10.7	3
4	F	—	—	—	—	—	—	—	20	24	20	21.3	3
7	F	—	—	12	—	15	6	15	3	6	6	9.0	7
8	M	—	—	—	—	—	—	—	28	—	—	28.0	1
10	M	—	—	—	—	—	—	—	—	—	16	16.0	1
11	M	—	23	—	—	—	—	—	—	—	—	23.0	1
12	F	—	—	—	—	—	—	19	—	—	28	23.5	2
13	F	—	13	23	7	16	12	7	7	9	12	11.8	9
14	M	—	—	—	—	—	—	10	—	—	—	10.0	1
17	M	—	—	17	5	11	4	25	13	5	19	12.4	8
18	F	—	12	8	—	—	—	—	—	—	—	10.0	2
21	M	—	—	—	—	14	—	—	—	—	26	20.0	2
24	M	—	—	—	6	—	—	—	—	—	—	6.0	1
25	F	—	26	19	25	8	11	9	9	3	2	12.4	9
28	F	—	—	—	22	—	—	—	—	—	—	22.0	1
29	F	—	—	—	—	11	—	22	3	12	4	10.4	5
Total responses		0	5	6	6	8	4	7	8	6	10		

TABLE 3—*Continued*

Group II-B

Subject No.	Sex	Test Trial No.										Mean Latency	No. Responses
		1	2	3	4	5	6	7	8	9	10		
1	M	—	21	5	1	—	—	—	—	—	—	9.0	3
4	F	13	—	4	13	2	5	5	29	27	1	11.0	9
7	F	—	13	19	15	—	—	10	—	—	—	14.3	4
8	M	—	—	—	—	—	—	23	—	—	—	23.0	1
9	M	—	5	5	8	6	—	—	—	24	—	9.6	5
10	F	5	3	2	13	8	4	—	—	—	19	7.7	7
11	M	8	—	—	—	—	16	—	—	—	—	12.0	2
13	M	6	—	25	—	14	27	17	—	—	29	19.7	6
14	F	2	2	2	2	2	2	2	2	2	2	2.0	10
17	M	—	—	—	—	—	—	18	—	—	—	18.0	1
18	F	—	—	—	13	—	—	9	8	—	2	8.0	4
20	M	—	—	—	8	—	5	—	—	—	—	6.5	2
21	F	1	5	3	3	5	—	24	4	1	3	5.4	9
22	F	—	—	23	—	—	25	4	—	—	24	19.0	4
27	F	—	4	—	—	—	—	6	2	3	—	3.8	4
29	F	7	8	16	5	4	3	2	9	2	1	5.7	10
Total responses		7	8	10	10	7	8	11	6	6	8		

TABLE 3—*Continued*

Group II-C

Subject No.	Sex	Test Trial No.										Mean Latency	No. Responses
		1	2	3	4	5	6	7	8	9	10		
1	M	13	28	8	—	17	—	—	18	—	—	16.8	5
4	F	—	—	—	—	26	—	—	—	—	—	26.0	1
7	F	12	27	9	12	5	—	—	—	28	—	15.6	6
9	M	—	—	21	—	—	—	—	19	—	—	20.0	2
10	M	—	25	—	—	—	—	—	—	—	—	25.0	1
11	M	—	—	—	—	26	—	—	—	—	—	26.0	1
13	F	9	7	27	26	—	—	—	—	12	—	16.2	5
16	M	1	—	3	2	3	2	1	4	—	—	2.3	7
17	F	2	6	4	1	1	5	4	4	5	5	3.7	10
19	F	—	6	—	3	—	—	—	—	—	—	4.5	2
20	F	—	2	—	4	17	—	—	—	—	—	7.7	3
26	F	—	—	5	—	9	—	—	—	—	—	7.0	2
27	F	—	9	14	—	—	—	—	—	—	—	11.5	2
28	M	—	—	—	—	—	—	—	—	—	10	10.0	1
30	F	—	4	—	7	—	4	3	29	—	7	9.0	6
Total responses		5	9	8	7	8	3	3	5	3	3		

Group III. (60 subjects)

A second control was run which gave information concerning the effect of the pairing of the light and tone which occurred in the reinforced test trials themselves. In this group, the tone was never presented during the test trials. This group was given the same instructions as Groups I and II, followed by ten 30-second *unreinforced* test trials. In these, the light was merely turned on for 30 seconds, or, if the subject pressed the key, until he released it. The tone was never presented to this group except during the instructions. The raw data for this group are given in Table 4. Twelve, or 20 percent of the 60 subjects in this group, gave one or more hallucinatory responses. The difference between this group and Group II may be attributed to the absence of reinforcement of the test trials in Group III.

A summary of the procedures for the control groups is given below. Each group was given the same original instructions as those given to Group I, followed immediately by the procedures listed:

Group II-A.
I. 10 30-second reinforced test trials
II. 'Rest period,' questions, and not-sure instructions
III. 10 30-second reinforced test trials

Group II-B.
I. 10 training trials
II. 10 30-second reinforced test trials

Group II-C. I. 10 30-second unreinforced test trials
II. 10 training trials
III. 10 30-second reinforced test trials

Group III. I. 10 30-second unreinforced test trials

TABLE 4

ORIGINAL DATA FOR CONTROL GROUP III

Numbers in the body of the table show latencies of hallucinatory responses in seconds for each test trial in which a response occurred. The absence of a latency figure indicates that no hallucinatory response occurred in that test trial. Only those subjects are represented who gave one or more responses.

Group III

Subject No.	Sex	Test Trial No.										Mean Latency	No. Responses
		1	2	3	4	5	6	7	8	9	10		
1	M	—	—	3	—	—	—	—	17	—	—	10.0	2
2	M	—	—	—	—	—	—	4	1	2	—	2.7	3
4	F	—	—	—	—	—	8	—	—	3	—	5.5	2
10	M	—	—	—	5	—	—	—	—	—	—	5.0	1
14	M	—	—	—	21	—	—	—	—	—	—	21.0	1
16	M	—	3	—	—	—	—	1	2	3	—	2.2	4
17	F	—	—	16	8	6	3	8	8	4	3	7.0	8
31	M	6	—	—	—	25	—	—	12	—	—	14.3	3
37	M	—	—	—	—	—	20	—	—	—	14	17.0	2
42	F	—	20	28	—	—	—	—	—	—	—	24.0	2
43	M	23	21	21	12	16	12	16	17	15	18	17.1	10
45	M	—	—	—	—	—	8	—	—	10	—	9.0	2
Total responses		2	3	4	4	3	5	4	6	6	3		

III. INTER-GROUP COMPARISONS

When the frequency with which each subject gave hallucinatory responses is tabulated, the distributions obtained were J-shaped for all of the control groups, and U-shaped for the experimental group and for both Group I and II-A when tested under not-sure instructions (see Table 5). This precluded the use of the critical ratio of the difference between means as a measure of the reliability of differences between groups. Instead, groups are compared on the basis of the proportion of subjects giving one or more responses. It may be stated categorically that none of the differences between mean latencies for any of the groups were statistically reliable. Consequently, all of the following discussion will center around the proportion of subjects responding in each group. The data for latency will be presented in the tables along with that for frequency but will not be discussed.

TABLE 5

DISTRIBUTIONS OF SUBJECTS GIVING FROM 0 TO 10 HALLUCINATORY RESPONSES IN 10 TEST TRIALS

The figures in the body of the table are the numbers of subjects giving each frequency of response. Figures for Group II are the sum of those for the sub-groups II-A, II-B, and II-C.

Group	Frequency of Response											Total N
	0	1	2	3	4	5	6	7	8	9	10	
I	8	5	3	3	6	2	2	2	1	3	5	40
II-A	13	6	3	2	1	1	0	1	1	2	0	30
II-B	14	2	2	1	4	1	1	1	0	2	2	30
II-C	15	4	4	1	0	2	2	1	0	0	1	30
II	42	12	9	4	5	4	3	3	1	4	3	90
III	48	2	5	2	1	0	0	0	1	0	1	60
Not-sure (N.S.) Instructions												
Group												
I (N.S.)	4	2	2	2	0	0	2	1	3	1	13	30
II-A (N.S.)	8	3	0	3	1	3	0	0	3	3	6	30

TABLE 6

SHOWING THE NUMBER AND PERCENTAGE OF SUBJECTS GIVING FROM 1 TO 10 HALLUCINATORY RESPONSES IN 10 TEST TRIALS, AND THE MEAN LATENCIES FOR EACH GROUP

Latency figures are the means of the subjects means for each group.

Group	Total N	Number of Subjects Responding	Percent Responding	Mean Latency (Seconds)
I	40	32	80.0	14.8
II-A	30	17	56.7	15.0
II-B	30	16	53.3	10.9
II-C	30	15	50.0	13.4
II	90	48	53.3	13.1
III	60	12	20.0	11.2
Not-sure (N.S.) Instructions				
I (N.S.)	30	26	86.7	11.0
II-A (N.S.)	30	22	73.3	11.9

Table 6 presents the number and proportion of subjects giving one or more responses in the 10 test trials in each group, and Table 7, the critical ratios of differences in the proportion of subjects responding. The sub-groups of Group II give very similar results and there are no reliable differences between any two of them. Consequently they are discussed below as a single group (Group II).

Eighty percent of the subjects in the experimental group gave one or more hallucinatory responses during 10 test trials. These hallucinations can be attributed to the training, *i.e.*, to sensory condition-

TABLE 7

INTER-GROUP DIFFERENCES AND CRITICAL RATIOS OF DIFFERENCES (D/σ_D) BETWEEN THE PROPORTIONS OF SUBJECTS GIVING ONE OR MORE HALLUCINATORY RESPONSES AND BETWEEN THE MEAN LATENCIES

Groups Compared	Proportion of Subjects Responding		Mean Latencies (Differences are in seconds)	
	Diff.	C.R.	Diff.	C.R.
I. Main Groups				
I vs. II	.267	3.24	1.7	1.08
I vs. III	.600	7.34	3.6	1.74
II vs. III	.333	4.52	1.9	0.96
II. Sub-groups of Group II				
II-A vs. II-B	.034	0.27	4.1	1.84
II-A vs. II-C	.034	0.27	1.6	0.62
II-B vs. II-C	.033	0.27	2.5	0.97
II-A vs. I	.233	2.12	0.2	0.10
II-A vs. III	.367	3.53	3.8	1.64
III. Not-sure (N.S.) Instructions				
I vs. I (N.S.)	.067	0.75	3.8	2.58
II-A vs. II-A (N.S.)	.166	1.37	3.1	1.57
I (N.S.) vs. II-A (N.S.)	.134	1.31	0.9	0.48

ing, if it can be shown that other factors would not produce similar results. Although the instructions were designed to reduce suggestion as much as possible, it must be recognized that with the exception of the repeated pairing of the conditioned and unconditioned stimuli, the experimental situation is very similar to that of the earlier suggestion experiments. Consequently, controls were run to determine the effects of suggestion in the instructions alone and in the instructions plus the test trials. In Group III, 60 subjects were merely given the instructions and 10 tests in which the light alone was turned on for 30 seconds. No tone was presented to this group except during the instructions, which were the same as for the experimental group. This group (III) serves as a control for the effects of suggestion in the instructions and also as a control for responses to extraneous noises which subjects might have confused with the tone. Twenty percent of the subjects gave responses in this group, this proportion being reliably different both from the 80 percent responding in Group I and the 53 percent in Group II. Critical ratios of the differences were 7.34 and 4.52, respectively. We may conclude that suggestion in the instructions and failure of the subjects to discriminate extraneous sounds from the tone cannot account for all of the subjects who gave hallucinatory responses in Groups I and II. However, the fact

that some subjects responded under the Group III conditions indicates that suggestion or external noises, or both, played some part.[2]

The procedure for Control Group II (all sub-groups pooled) involves 10 reinforced test trials preceded either by no training or by 10 training trials. The proportion of subjects responding in this control group was reliably different from that in Group III (critical ratio, 4.52). There is a more interesting difference which is also reliable (critical ratio, 3.53) between sub-group II-A and Group III. Group II-A and Group III differed only in that the test trials for Group II-A were reinforced, while those for Group III were not. We may conclude from this that the pairing of the light and tone in the test trials alone is effective in causing hallucinations in some subjects.

The proportion of subjects responding in Group II (53 percent) was also reliably different (critical ratio, 3.24) from the proportion responding in Group I. Since the results for these two groups represent similar tests given before and after 60 training trials, we may conclude, finally, that the pairing of the light with the tone was effective in causing hallucinations in some subjects. In other words, auditory hallucinations were produced by a sensory conditioning procedure.

Progressive Changes During Test Trials

Table 8 presents the frequency of response on successive test trials for all groups. Only one of the groups, Group II-A, shows a reliable trend. The difference in the proportion of subjects giving hallucina-

TABLE 8

FREQUENCY OF HALLUCINATORY RESPONSE IN 10 SUCCESSIVE 30-SECOND TEST TRIALS

Figures for Group II are the sum of those for sub-groups II-A, II-B, and II-C.

Group	No. Subjects	Test Trial No.									
		1	2	3	4	5	6	7	8	9	10
I	40	19	12	18	13	19	16	18	17	16	17
II-A	30	0	5	6	6	8	4	7	8	6	10
II-B	30	7	8	10	10	7	8	11	6	6	8
II-C	30	5	9	8	7	8	3	3	5	3	3
II	90	12	22	24	23	23	15	21	19	15	21
III	60	2	3	4	4	3	5	4	6	6	3
Not-sure (N.S.) Instructions											
I (N.S.)	30	20	22	18	20	17	19	20	20	19	19
II-A (N.S.)	30	13	15	11	14	15	16	16	14	16	12

[2] Some evidence that these responses were due to subjective factors was obtained: As an after-thought when the experiment was completed, the Laird Personal Inventory, form B-2 was given to those subjects still available, and for 26 subjects in Group III, including 5 who reported hearing a tone, a bi-serial *r* of .81 was obtained for those who did and those who did not hear the tone *vs.* the Laird scores. This lead is being followed up in another experiment.

tory responses in the first and last trial for this group is reliable, the critical ratio being 3.88. This difference suggests that learning occurred during reinforced test trials and may account for the absence of extinction effects in the reinforced test trials given to Group I following training.

TABLE 9

FREQUENCY, LATENCY AND DURATION OF 20 UNREINFORCED 60-SECOND TEST TRIALS GIVEN TO THIRTY SUBJECTS OF GROUP I

The frequency figure is the number of subjects responding on a given trial. Latency and duration means are computed on the basis of those responses which occurred, *i.e.*, N = the frequency.

Trial No.	Frequency	Mean Latency	Mean Duration
1	18	12.3	5.4
2	20	12.3	5.2
3	18	15.3	4.4
4	19	13.7	4.8
5	19	15.1	5.1
6	16	12.1	4.9
7	18	12.7	5.4
8	21	15.4	5.1
9	17	16.2	5.7
10	21	17.1	6.2
11	20	19.8	4.8
12	20	13.3	5.8
13	17	11.6	6.3
14	19	16.1	5.4
15	19	14.2	5.4
16	17	16.2	5.5
17	21	15.5	5.4
18	19	14.9	7.6
19	18	12.2	6.2
20	16	14.7	5.7

Table 9 shows the number of subjects responding, and the mean latency and duration of response in the 20 unreinforced 60-second test trials given to 30 subjects of Group I. These are essentially extinction trials since the unconditioned stimulus is omitted. There is no evidence of a progressive decrease in frequency or duration, nor an increase in latency. Under these conditions extinction apparently occurs either very slowly or not at all. A further test of this conclusion is necessary, however, since these trials occurred under the not-sure instructions and there is no comparable control to assure that the initial responses were stronger than would be obtained by suggestion alone.

Effects of Not-sure Instructions

In answer to the question, "Do you usually press the key as soon as you hear the tone or do you wait to make sure?," 37 of the 40 sub-

jects in Group I and 23 of 30 in Group II-A reported that they waited to make sure. The original instructions were apparently interpreted to mean that the subjects should try not to respond until they were sure of hearing the tone. It was noticeable, although no quantitative data were obtained, that the subjects who reported that they pressed the key immediately had less difficulty or at least reported less difficulty in discriminating the presence of the tone. Many of these seemed not to understand the question concerning difficulty in recognizing the onset of the tone. For them there was apparently no question concerning the onset—the tone was either heard or it was not heard. On the other hand, the subjects who reported themselves as being very strict in their criterion of hearing the tone also reported the greatest difficulty in making the discrimination.

The not-sure instructions were introduced with Groups I and II-A to determine whether some subjects were hearing the tone as an hallucination but were recognizing it as being of subjective origin and thus not pressing the key. Although both groups showed an increased frequency of response and decreased latency under these conditions, the differences were not reliable.

Discussion and Conclusions

It has been demonstrated that auditory hallucinations can be produced by pairing a cue stimulus with a tone in what is essentially a conditioning situation. It was shown that the repeated pairing of stimuli involved in this procedure was an effective factor in addition to those factors present in the earlier sensory suggestion experiments. It is not contended, however, that the phenomenon obtained in this experiment is 'conditioning' in the usual sense. The chief similarity between the auditory hallucinations evoked by a light stimulus in the present experiment and the ordinary conditioned response lies in the antecedent conditions: two stimuli were repeatedly paired, and as a result of this pairing, one of the stimuli presented alone came to evoke the response previously evoked by the other.

If conditioning is defined in terms of the situation in which it occurs, then the hallucinations were 'conditioned'; if, however, conditioning is defined in terms of the functional characteristics of the phenomenon, then further information is necessary. Some of the evidence obtained suggests the presence of important differences between the results of the present experiment and the characteristics of conditioned responses. For instance, the latency, or interval between the onset of the light and the subjects' responses differs considerably, both in magnitude and variability from that obtained in the typical motor conditioning experiments (see Tables 2, 3, 4, and 6). Moreover, latency does not seem to be correlated with amount of training

or frequency of response (Table 6, last column). There was no evidence of a gradual acquisition or extinction, although the evidence concerning extinction is not conclusive under the conditions of the present experiment. The preceding considerations suggest that something other than conditioning is involved. However, it is not necessary to reach a definite conclusion here. Further experiments, some of which are planned or in progress, are needed to study the occurrence of extinction and spontaneous recovery, the effect of knowledge of the subject concerning the objective presence of the tone,[3] the relationships to personality variables, the possibility of obtaining similar results in other sensory modalities, etc. Until further experiments demonstrate that the phenomenon is definitely not conditioning, a conditioning terminology may be used tentatively, as a matter of convenience.

Leuba (**10**) has suggested that sensory conditioning may account for imagery, *i.e.*, that images are conditioned sensations. Such a conclusion might be received more charitably by strict behaviorists if it were worded somewhat differently; *e.g.*, that conditioned sensations or perceptions are what the early introspective psychologist called images. Images, however, were reported to differ from sensations in the dimensions of intensity and clearness, which permitted a discrimination between the two. A subject could discriminate easily between 'seeing' and 'imagining' a visual stimulus. However, an experiment performed by Perky in 1910 (**14**) showed that this was not always true. She had subjects 'imagine' a colored object upon a ground-glass screen, then projected a faint, out-of-focus image of the object onto the screen from behind. Under these conditions she found that subjects did not discriminate between the image they believed themselves to be producing and the physical stimulus, although in many cases it was apparent that the form or position of the physical stimulus influenced or changed the nature or position of the imagined figure. Similar results have been obtained recently by J. G. Miller (**12**), these two experiments indicating clearly the existence of a range of characteristics of a physical stimulus such that an objective stimulus is not discriminated from an 'imagined' stimulus.

The present experiment supplements this conclusion with a converse statement, *i.e.*, that under certain conditions images or centrally aroused perceptions may be obtained which cannot be discriminated from perceptions of physical stimuli having certain characteristics. It would seem plausible to assume that under other conditions, con-

[3] Four psychologists were run through the procedure of Group I. In spite of their knowledge of the purpose of the experiment, and of the general procedure to be used, one of the four gave four responses, and another gave nine responses in the 10 test trials. This rather unexpected result supports the conclusion that the repeated pairing of light with tone has an effect distinct from that of suggestion or expectation.

ditioned sensations might be discriminable from the perception of the physical stimulus. When so discriminated, they would be classed as images, and when not discriminated, they would be classed as hallucinations. This would justify the term 'hallucination' as it is used in this paper.

Summary

1. A tone with a gradual onset and decline, used as an unconditioned stimulus, was paired for 60 trials with a light used as a conditioned stimulus. Thirty-two of 40 subjects under these conditions reported hearing the tone when the light was presented alone.

2. Appropriate control groups demonstrated that the effect was not due entirely to suggestion in the instructions or test trial conditions.

3. No conclusion was reached as to whether the phenomenon was 'true conditioning' or not on the basis of present evidence.

4. Leuba's contention that conditioned sensations may be identical with what has been called images was supported, but it was concluded that the conditioned sensations of the present experiment were better described as hallucinations since they were not discriminated from perceptions of a physical stimulus.

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References

1. Bogoslovski, A. I., An attempt at creating sensory conditioned reflexes in humans, *J. exper. Psychol.*, 1937, **21**, 403–422.
2. Brogden, W. J., Sensory pre-conditioning, *J. exper. Psychol.*, 1939, **25**, 323–332.
3. Brown, W., Individual and sex differences in suggestibility, *Univ. California Pub. Psychol.*, 1916, **2**, 291–430.
4. Cason, H., Sensory conditioning, *J. exper. Psychol.*, 1936, **19**, 572–591.
5. Cole, L. E., A comparison of the factors of practice and knowledge of experimental procedure in conditioning the eyelid response, *J. gen. Psychol.*, 1939, **20**, 349–373.
6. Ellson, D. G., Spontaneous recovery of the galvanic skin response as a function of the recovery interval, *J. exper. Psychol.*, 1939, **25**, 586–600.
7. von Fieandt, K., Ein Neues Invarianzphänomen der Farbenwahrnemung, *Ann. Acad. Sci.* (Finland), 1938, **41**, pp. 245. (English summary.)
8. Garvey, C. R., A study of conditioned respiratory changes, *J. exper. Psychol.*, 1933, **16**, 471–503.
9. Kelley, E. L., An experimental attempt to produce artificial chromaesthesia by the technique of the conditioned response, *J. exper. Psychol.*, 1934, **17**, 315–341.
10. Leuba, C., Images as conditioned sensations, *J. exper. Psychol.*, 1940, **26**, 345–351.
11. Lumsdaine, A. A., Conditioned eyelid responses as mediating generalized conditioned finger retractions, *Psychol. Bull.*, 1939, **36**, 650.
12. Miller, J. G., Discrimination without awareness, *Amer. J. Psychol.*, 1939, **52**, 562–578.
13. Mowrer, O. H., Preparatory set (expectancy)—a determinant in motivation and learning, *Psychol. Rev.*, 1938, **45**, 62–91.
14. Perky, C. W., An experimental study of imagination, *Amer. J. Psychol.*, 1910, **21**, 422–452.
15. Scott, H. D., Hypnosis and the conditioned reflex, *J. gen. Psychol.*, 1930, **4**, 113–130.
16. Seashore, C. E., Measurements of illusions and hallucinations in normal life, *Stud. Yale Psychol. Lab.*, 1895, **2**, 1–67.
17. Shipley, W. C., An apparent transfer of conditioning, *J. gen. Psychol.*, 1933, **8**, 382–391.

VIBRATORY SENSITIVITY AS A FUNCTION OF SKIN TEMPERATURE

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Introduction

Conjectures concerning the mediation of skin sensitivity usually take as a point of departure the stimulus, lead to the receptor, and from here abruptly advance to the impulse aroused by the stimulation. It would seem that an important transition is minimized, namely the step between the activation of the receptor and the initiation of the impulse. In what manner does this transition occur?

One of the most adequately controlled forms of cutaneous stimulation is the vibratory one. It would then seem profitable to use it in an investigation directed at the neglected intermediate step.

When considering various hypotheses one must not exclude the possibility of cutaneous sensations originating in receptors by some chemical action. With this assumption in mind, one would then expect that temperature should have some effect on the process involved, since temperature affects most reactions of a chemical nature. If it were to have some effect what would one expect it to be? It has been found in the majority of chemical reactions that substances will unite or decompose more readily when heated and the entire reaction will increase in velocity. If this be the case and a chemical reaction is the basis of vibratory sensations, then it too should have its velocity increased and the components involved should react more readily. What would this mean in terms of vibratory sensitivity? If the foregoing assumptions are correct it would seem that under conditions of increased warmth a lower intensity of stimulation would be sufficient to set off the reaction (or disturb the equilibrium of the resting state) than would be required at a lower temperature. In other words, intensity thresholds should be lowered by the application of warmth and raised by the administration of cold.

A simple test of this suggested itself. If a vibrating needle were placed on the skin at a sub-threshold level, then warming the skin should bring about sensations of vibration if the intensity thresholds were lowered while the actual amplitude of the needle's vibration remained unchanged.

It was found that the above assumptions were apparently correct. Using as a small heat stimulus an electric cigarette lighter which was

* The writer wishes to express his thanks to Dr. Frank A. Geldard for his very helpful advice.

connected to a 6–8 V. transformer, sensations of vibration were elicited when a stimulus needle was vibrating at a sub-threshold amplitude and the heater was allowed to warm an area of the skin in the vicinity of the vibrating needle. The most striking phenomenon of this experiment was that the sensation of vibration was localized not at the needle but in the warmed area. This could be explained on the foregoing hypothesis as follows: The vibratory disturbances travel through the skin for some distance away from the stimulus needle; this has been shown to be the case and the writer, in an earlier paper (**16**), has described a method of ascertaining it. Although these vibrations are at a level below that necessary normally to stimulate the receptor for vibration, with warming the skin the threshold for this sensation is lowered. The disturbance present thus becomes a supraliminal one.

This result seemed indeed indicative of the need for further investigation. Here was not only the possibility of determining some of the facts concerning the chemical nature of skin sensitivity, but there was also afforded the possibility of ascertaining the effect of temperature changes on pressure sensitivity and thus determining whether the relation between the two, vibratory and pressure sensitivities, was upheld. This conclusion concerning the identical mediation of the two was drawn in a previous paper by the writer (**16**), and has, indeed, been urged since the pioneer work of von Frey (**5**).

As Geldard (**6**) has pointed out in a recent paper, there has been a great deal of controversy concerning the mediation of vibratory sensitivity. Contentions as to the receptor involved have been numerous. However, if it were found that pressure and vibratory sensitivities showed a common dependency on temperature it would seem to be additional evidence of the correspondence of mechanisms involved. If, however, it were found that the functions were unrelated it would certainly be indicative of a divorcement of the two.

The literature directly pertaining to the problem at hand is very sparse and that which was found will be discussed later in conjunction with the results of the present experiment.

Apparatus

The apparatus used in this study was exactly the same as that previously described by the writer (**16**). In brief, it consisted of a small sewing needle 0.28 mm in diameter with a fairly sharp point, attached to the armature of an electromagnetic speaker unit. This served as the stimulus. It was activated by means of a beat-frequency oscillator. The impulses from the oscillator were unamplified for the majority of the work, which was done at a frequency of 256 cycles; however, for a frequency of 900 cycles amplification was found necessary and this was accomplished with a two-stage push-pull amplifier. Included in the circuit was an L-type attenuator between the amplifier and the oscillator. The intensity of the vibration was controlled by means of a potentiometer which was calibrated in terms of amplitude of swing of the needle; thus a setting on the potentiometer could be directly translated into an amplitude in micra.

An arm rest was provided for the subject's arm, which was the area stimulated, and all stimulations were observed through a binocular microscope using a magnification of 30 diameters. With the aid of an eye-piece micrometer, skin gradients could be kept constant and it was relatively easy, with the use of the microscope, to relocate the same 'spot' previously stimulated.

The heating device employed was a 600 watt nichrome coil, mounted in a metal cylinder 9 cm in diameter and 16 cm long. The latter was mounted on a metal shaft with an adjustable base so that any desired angle with the base could be obtained in order better to direct the heat. An asbestos shield was employed to limit the area to be warmed. This was placed 'tunnel fashion' over the subject's arm and with it the area under direct stimulation by heat was from 50 to 75 square cm, depending upon the dimensions of the forearm of the subject. However, there was a spread beyond the region directly exposed to the radiant heat stimulus.

The cooling unit was a hollow metal cylinder 6.5 cm in diameter and 12 cm in length. One end was completely covered by a wire screen which was soldered to the inner surface of the cylinder. The mounting was similar to that described for the heating unit. Dry ice was placed in the cylinder and by bringing it close to the skin 'radiant cooling' was effected without any contact of a solid cold object with the skin. The skin area directly cooled was approximately 30 to 50 square cm. This is a method employed by Hardy and Oppel (9) in some studies on temperature sensitivity and was found to be very adequate in the present work.

The skin temperature was measured by means of a thermocouple attached to a galvanometer (G-M Electrical Thermometer and Temperature Rise Indicator) which was calibrated directly in degrees centigrade, and which could be accurately read to 0.25 degrees. The thermocouple was mounted in cork which was fitted into a wooden handle. This made manipulation of the instrument very convenient and insured against external changes having any effect upon the skin temperature readings.

The room in which all the research was carried out was of fairly constant temperature, never varying over 3 degrees centigrade for a period of six months, the duration of the actual experiment

Procedure

The experimental work can be conveniently divided into five parts: (1) the effect on vibratory thresholds of warming the skin; (2) the effect of cooling on these thresholds; (3) the effect on vibratory thresholds of cooling and subsequent warming of the skin; (4) the effect of warming on vibratory thresholds using test frequencies other than 256 cycles; (5) controls.

In the first two parts of the experiment six subjects were used, four males and two females. In Part III one male was used, and in Parts IV and V one male and one female, eight subjects in all. Three of these subjects were trained observers of vibratory sensations, the other five were given training periods before any experimental work was done. In some cases the volar side of the arm was used as the area stimulated whereas in other instances the dorsal surface was employed. The reason for using the two different sides of the forearm was that in some cases the subject had very fine hairs which could not be avoided when stimulating and could not be satisfactorily removed by either shaving or cutting. In these cases the dorsal surface was used and the hairs cut with scissors. If shaving were employed to remove the hairs, the subject did so the night before the experimental session since it was found that shaving just prior to an experimental period left the skin very irritable. It should be mentioned that the two sides of the arm respond in the same way to vibratory stimulation with the exception that the volar surface is usually more sensitive (Gilmer, 8). However, this had no effect in the present investigation.

Regardless of whether the volar or dorsal surface of the forearm were used, stimulation was just proximal to the wrist. After a hair-free area was found, a small ink mark was made for orientation. A 'spot' was then arbitrarily chosen in the vicinity of this mark and stimulated by raising the arm to the needle by means of a screw system on the arm rest. The movement of the arm was so small as to give no accompanying kinesthetic sensations. This 'spot' could easily be relocated by its relative position to the orienting ink mark and its position on the 'hills' and 'furrows' in the skin.

If a response of 'prick' were evoked upon stimulation with the stationary needle the 'spot' was discarded and another one obtained. If any other report were given upon stimulation it was noted and vibratory intensity thresholds were then taken. These were measured by the method of minimal change. Five such thresholds were obtained and the average of these was taken as the normal threshold of the 'spot' under observation. The skin temperature of the

area in which the 'spot' lay was then obtained by means of the thermocouple. A rest period of ten minutes was allowed the subject. After this interval the subject again placed his arm in the rest and the area was either warmed or cooled, depending upon the design of the experiment. During the warming or cooling, thresholds were again taken on the same 'spot.' However, only ascending measures were obtained since the area was changing in temperature and time limitations did not permit the performance of a descending series. Immediately after having reached a threshold, temperature measures were again recorded and another series was undertaken. This was continued until the temperature change caused discomfort to the subject or he reported fatigue from maintaining a relatively fixed position. In this way normal vibratory thresholds were obtained on a given 'spot' and also thresholds on the same 'spot' with either increasing or decreasing temperature.

Results

Part I.—The effect on vibratory thresholds of warming the skin.

The results of this part of the investigation, as in all other parts, will be presented graphically rather than in tabular form.[1] The graphs are constructed by equating, on the ordinate, the normal threshold to 100 percent and determining the relative percentage of the other thresholds obtained; the abscissa is the rise in skin temperature above normal, that is, 0 is then normal skin temperature. The threshold value was then plotted against the skin temperature at which it was obtained.

Figure 1 shows the results of this part of the work. It can be seen that with increasing temperature there is a drop in vibratory thresholds which falls to a minimum. With further heating there is a subsequent rise which goes well above the original normal thresholds. The function for all subjects is strikingly similar, the low point being at an average of 36.9°C. with a range from 35°C. to 38°C.

Part II.—The effect on vibratory thresholds of cooling the skin.

Figure 2 shows the effect obtained on vibratory thresholds with cooling the skin. It can be seen that with decreasing skin temperature vibratory thresholds show a continuous rise, going well above normal values. The maximum drop in skin temperature ranged from 4.5°C. to 13°C. The same six subjects were again used and once more it is obvious that the function here obtained is similar from subject to subject.

Part III.—The effect of cooling and subsequent warming on vibratory thresholds.

In order to obtain a complete curve of the entire temperature function normal thresholds were taken on a given 'spot'; the area was then cooled and successive threshold measurements taken. When the skin temperature returned to normal, as evidenced by thermal measurements, and vibratory sensitivity was restored to its original level, as indicated by threshold measurements, a warming series was taken on the same 'spot.' Two different 'spots' were manipulated in this way.

[1] The actual measurements in tabular form are on file at the Psychological Laboratory of the University of Virginia.

Figure 3 shows the results of these experiments giving the entire temperature function. This figure is analogous to a combination of the first two parts of this experiment, the difference being that now precisely the same receptors are involved in the two parts of the

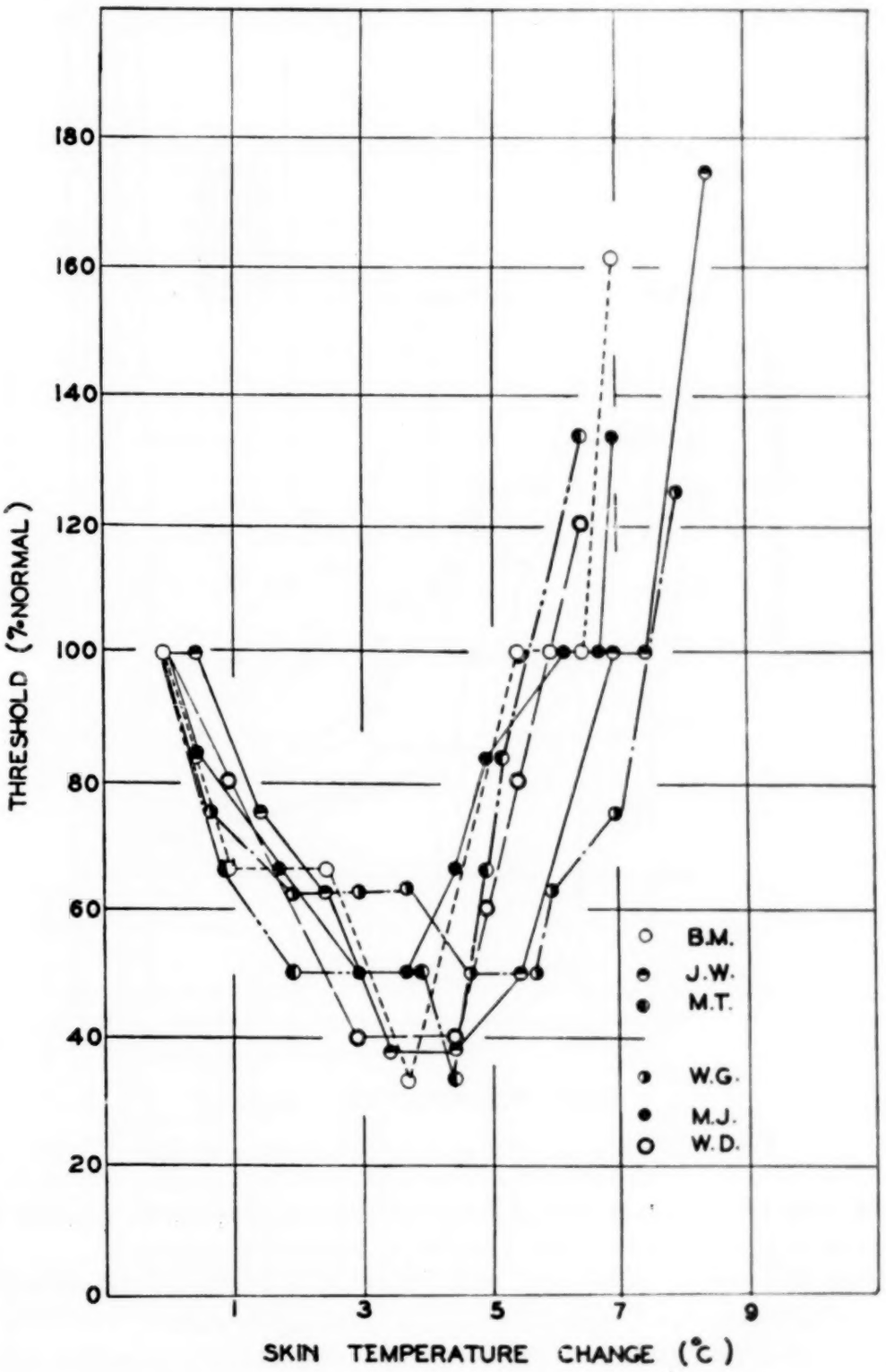

FIG. 1. Effect on vibratory thresholds of increasing temperature.

curve. Starting at the low temperature end of the figure, one finds a decrease in vibratory thresholds to about 4°C. above normal skin temperature, at which point these thresholds reach a minimum and rise with subsequent increase in temperature. It should be men-

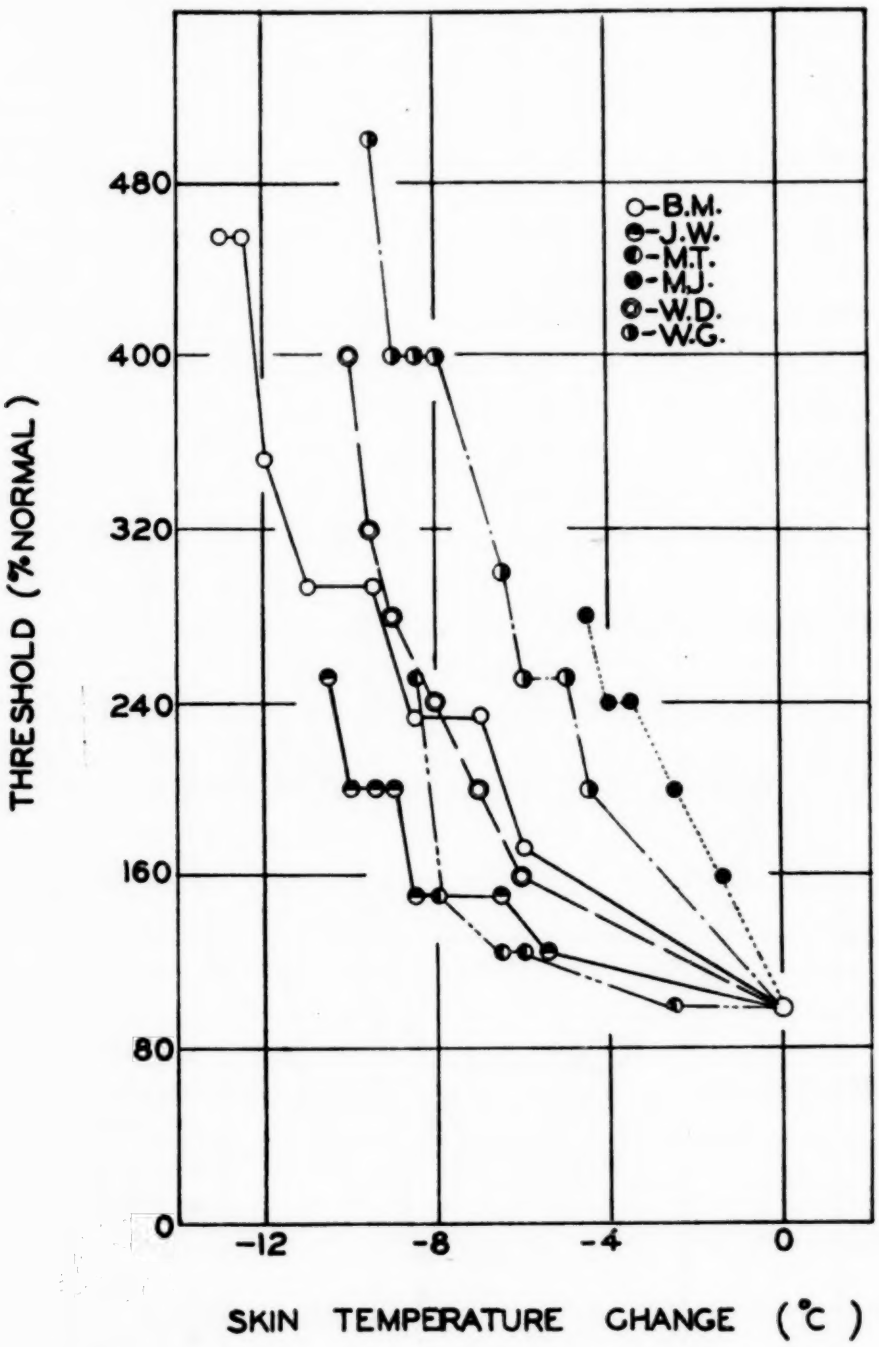

FIG. 2. Effect on vibratory thresholds of cooling the skin.

tioned that the point of coincidence of the two curves at normal temperature is forced due to the graphical method employed.

Part IV.—The effect on vibratory thresholds of warming the skin, using stimulus frequencies other than 256 cycles.

This division of the investigation was undertaken to insure against the possibility of the results being peculiar to a given frequency,

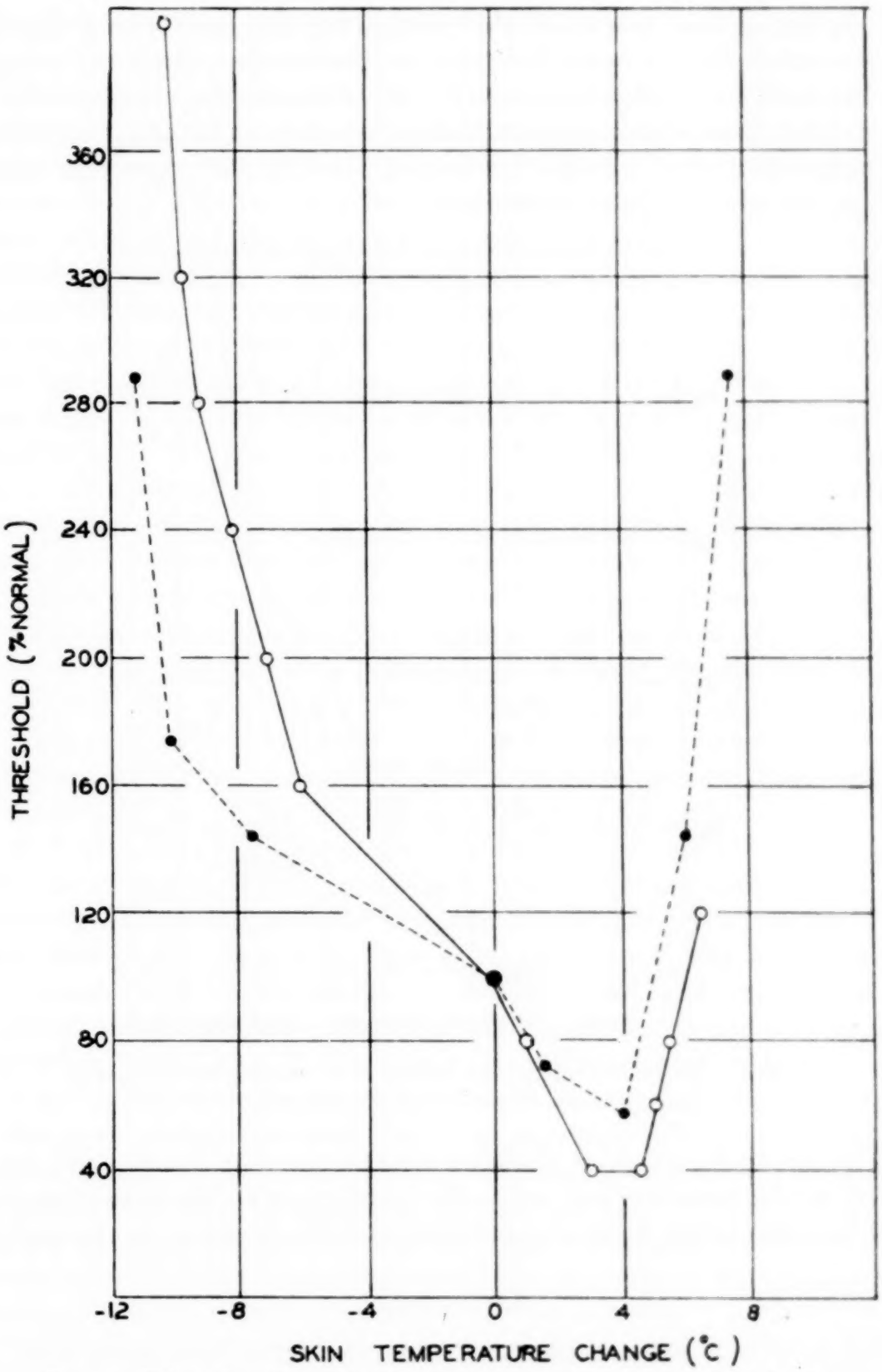

FIG. 3. Effect of cooling and subsequent warming on vibratory thresholds.

hence 100 cycles and 900 cycles were used as stimulus frequencies.[2] The procedure was exactly the same as in Part I.

[2] The selection of the values was somewhat arbitrary, though significantly higher and lower frequencies seemed desirable and the characteristics of the vibrator were such as to insure good sinusoidal wave-form at 100 and 900 cycles. They were also chosen to avoid the multiple and sub-multiple relation with 256 cycles.

Figure 4, the graph of the results of this portion of the work, shows precisely the same function as that earlier obtained using 256 cycles as the stimulus frequency. It is interesting to note the very close correspondence between these two curves for the two different frequencies and it should be further mentioned that the absolute values are also in close agreement.

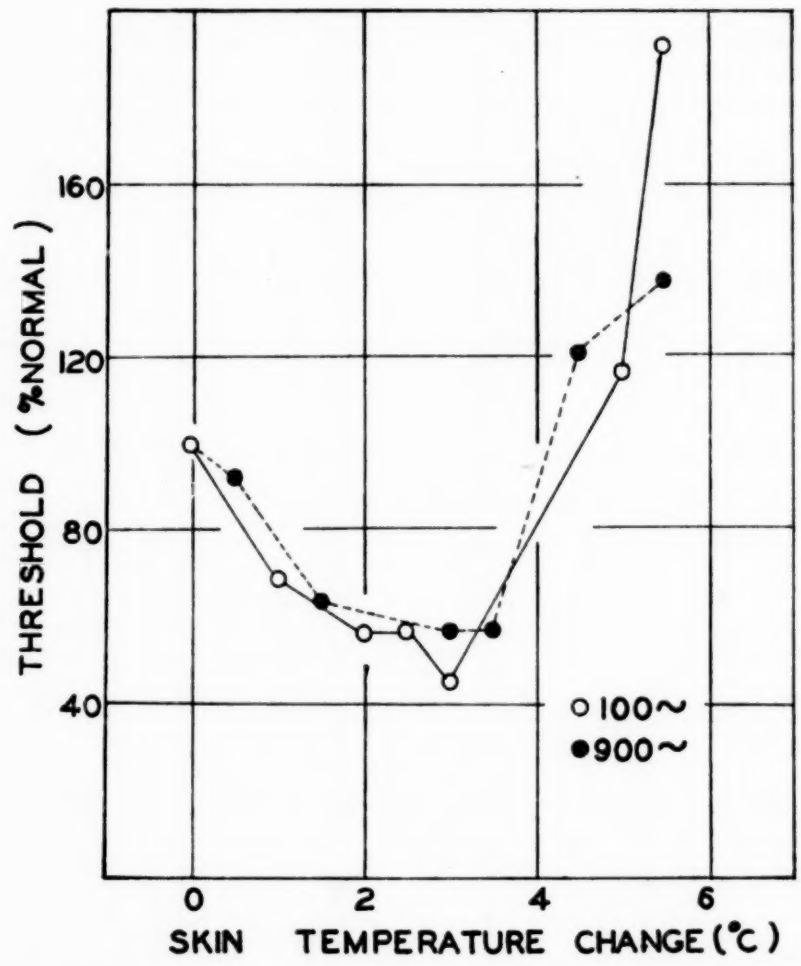

FIG. 4. The effect on vibratory thresholds of warming the skin, using stimulus frequencies of 100 and 900 cycles.

The controls, or fifth division of the present study, will be discussed in the next section, not only in relation to the investigation at hand but also in the light of some other studies bearing on the problem.

DISCUSSION

Up to this point one can see, from the data presented, that with increasing temperature there is a drop in vibratory thresholds to a minimal value at which point, with further heating, there is a subsequent rise in these thresholds. This is true of all frequencies investigated as well as of all subjects tested, there being not a single inversion.

This function seems to be in harmony with predictions earlier made concerning the chemical origin of the sensation. The subse-

quent rise, although not previously hypothesized, will be later shown to accord with chemical theory.

There was, however, the possibility of the effects of temperature on vibratory sensitivity being purely mechanical in nature. If with increasing temperature there were afforded, by some means, an enhancement of skin conductivity of the vibratory disturbances it would permit their spread to a more sensitive area with a consequent lowering of thresholds. This assumption was tested in the following manner: placing the vibrating needle at a given point on the skin and observing the movement of a hair some distance away, it is possible from the measurement of the amplitude of this movement to determine whether or not there is greater spread with increasing skin temperature. This was accomplished by training the microscope on a hair illuminated by the 'Strobotac,' a stroboscopic instrument, slightly mistuned to the frequency of the needle so that it 'stopped down' the apparent motion of the hair to a very slow one, permitting the amplitude of swing of the hair to be measured on the eye-piece micrometer. This is a method suggested by Geldard (7). The skin gradient of the needle was maintained constant by aligning the top of the hair with one edge of the micrometer scale, thus any change in skin gradient would cause the hair to move above or below the scale, depending upon whether the arm were moved into or away from the needle. With a fixed amplitude of vibration of the needle it was found that the amplitude of movement of a hair 45 mm away from the stimulus needle remained constant regardless of the temperature. Warming and cooling was obtained in the manner previously described. It would then seem that there was no increase in the transmission of the vibratory disturbances with increasing temperature, since if there were, one would expect the amplitude of the movement of the hair to increase with increasing spread.

Although the above procedure tested the possibility of the drop in vibratory thresholds being induced mechanically it still would not explain the subsequent rise with increasing temperature, even had it explained the descending phase of the curve. Probably the most obvious possible cause for this rise would be fatigue. As a consequence of successive stimulation, adaptation might have taken place resulting in a rise of thresholds.

This suggestion would seem obviated, however, on the basis of other investigations of the adaptation process. Wedell and Cummings (15) as well as Kampik (10) have found that vibratory sensations do adapt, but only slowly, after long, continuous stimulation. Momentary cessation of stimulation results in recovery and the fatiguing effects are abolished. In the present work there was no long, continuous stimulation; further, there was an even greater in-

terval of time between consecutive threshold series. This, then, would apparently eliminate the possibility of the terminal rise being a product of fatigue.

Another possible explanation of the results lay in the operation of some vascular action, which might, through alteration of blood volume, change the density and thus the vibratory conductivity of the tissue. It is well known that vaso-contraction occurs with increasing cold whereas increasing warmth results in vaso-dilatation. It was conjectured that these mechanisms might be involved in the alteration of vibratory thresholds found in conjunction with changes in skin temperature.

It has been shown by Brown-Séquard (**3**) that vaso-contraction takes place in one arm when the contralateral arm is placed in cold water, and Wagoner and Havemann (**14**) have shown that vaso-dilatation occurs when the contralateral member is placed in warm water. Further, Wagoner (**13**) has reported that there is no concomitant rise in the temperature of the arm which is not directly stimulated. This, then, affords a very good basis for testing the above assumption, namely that the effects gained with temperature are due to some vascular action.

In obtaining thresholds on a given 'spot' and retaking vibratory thresholds on the same 'spot' while the contralateral arm was in warm or cold water, it was found that insignificant differences [3] in thresholds resulted. None of the differences even approached statistical reliability, and in several cases the extremely small differences which did exist were found to be in the opposite direction to that which one would predict if vascular changes were the basis of the results obtained earlier.

This control also showed that the terminal rise in thresholds in the case of warming and the continual rise with cooling was apparently not due to attentional factors, for here temperature sensations were elicited which were as strong as those in the actual procedure, yet they had no distracting effect, as shown by the inconsequential change in thresholds in the above mentioned investigation.

It was very fortunate that these controls were undertaken, for a study by Allers and Halpern (**1**) was found in the literature upon which these have direct bearing.

Using carefully controlled methods these investigators determined the effect of temperature on pressure sensitivity. Their findings are very similar to those of a much earlier investigator, Moore (**12**). However, their procedure had a great many refinements which were not present in the earlier work.

[3] The figures for this work are also on file at the Psychological Laboratory of the University of Virginia.

They found that pressure thresholds decreased to a minimum with increased warmth and subsequently rose from this minimum with further heating. Their minimum was found to lie between 36°C. and 38°C. The results of this investigation are presented graphically by them and a reproduction of their curves is shown in Fig. 5.

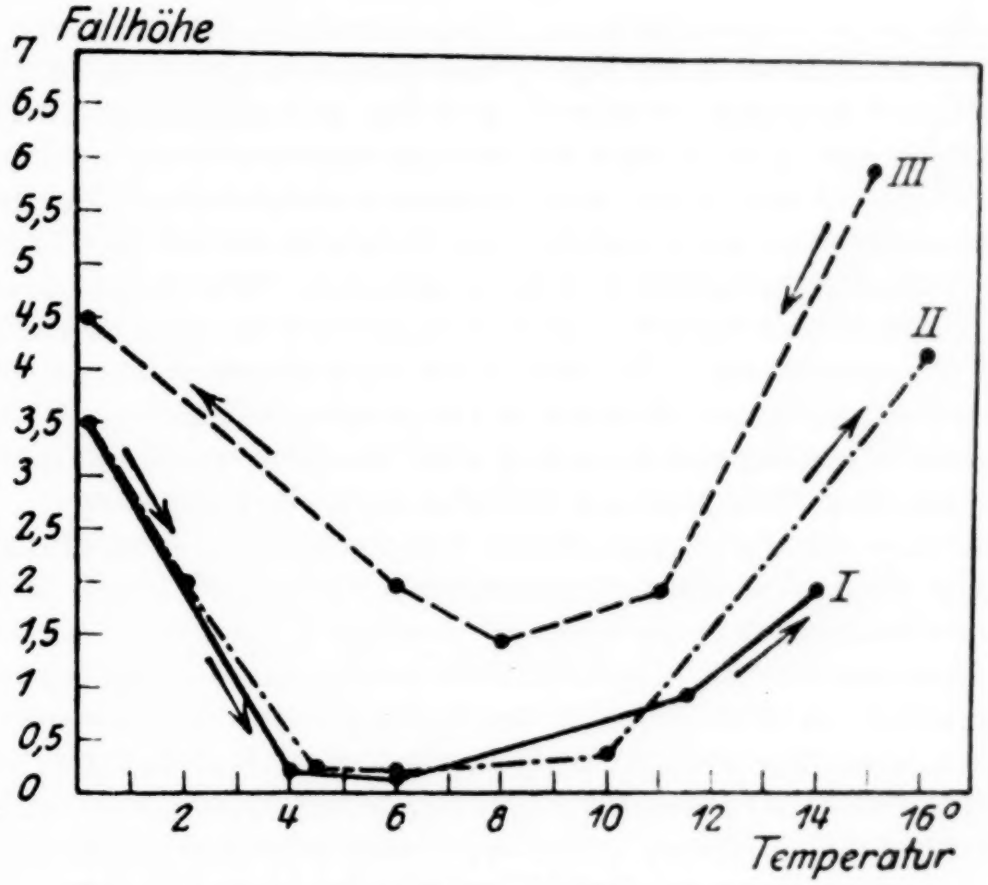

FIG. 5. Curve taken from Allers and Halpern. Effect of increasing warmth on pressure thresholds.

However, they objected to Moore's interpretation of his results on the basis of some further considerations. They hypothesized that the hyperemia caused by heating the skin resulted in greater tension (caused by vaso-dilatation) and it was this tension which caused the changes in pressure thresholds. In other words, it was a purely mechanical effect. Moore had hypothesized his results as being derived from some basic chemical action.

The stretching of the skin made it more stimulable up to a certain point, contended Allers and Halpern. Beyond this point the stretch in the skin gave rise to pressure sensations which had to be overcome if the pressure stimulus were to be felt. Hence a greater stimulus intensity was necessary to reach threshold.

However, if there were this stretching effect with warming the skin one would expect that the vibratory disturbances in the skin would travel much farther, since it was found by the present writer

(**16**) that transmission distances were greatly enhanced in skin stretched with adhesive tape. Yet it will be remembered that such was not the case when the skin was warmed. Further, the experiment involving contralateral stimulation showed no change in vibratory thresholds although the mechanism for stretching was present (vaso-dilatation) and should have been manifest had such a factor been the basis of the results.

Upon examination of Fig. 5 one may readily observe that the similarity of function between it and Fig. 1 is remarkable. Particularly is this the case in view of the fact that one deals with pressure sensitivity and the other with vibratory sensitivity. Further, the two investigations were carried out in ignorance of each other and vastly different techniques were employed. The experimental results of Allers and Halpern seem unequivocal in spite of their theoretical interpretations. In view of the very close agreement between their curves and those obtained in the present work it would indicate more than ever that one is dealing with the same mechanism whether he stimulates with a pressure stimulus or a vibratory one.

Not only are the curves similar but the optimal temperatures are also very close, that for pressure being between 36°C. and 38°C. and that for vibration ranging from 35°C. to 38°C.

In contrast to Allers and Halpern study, Fischer (**4**) reported an investigation on the effect of temperature on 'prickle sensation.' This is a sensation aroused by stimulation with alternating current and gives rise to a feeling very similar, qualitatively, to that evoked by a mechanical vibratory stimulus. His research was a study of the effect of temperature on the differential frequency limen. The intensity for all stimulation was, of course, above threshold and maintained at a constant level. Using this procedure, Fischer found that the difference limen was the same regardless of the temperature, and so concluded that temperature had no effect on this form of cutaneous sensitivity.

It would seem that the experiment done in the present investigation, determining the effect of temperature on vibratory thresholds at different frequencies, would be pertinent at this point. It will be remembered that regardless of the frequency (of those investigated) the function with varying temperature was the same, so that if intensity were maintained constant, as was the case in Fischer's study, and it were at a level well above threshold, one would never discover the effects of temperature since the entire function would be shifted to a new level and the internal relationships between the various frequencies would still be maintained. Thus, for example, if difference limens were obtained at 40°C. and 20°C. they should be the same since the difference between the two curves of frequency would be precisely

the same at these two temperatures although the absolute values of the measurements would be quite different had stimulus thresholds been taken.

Bellows (**2**) investigated the effect of different temperatures of air blasts on skin fllicker frequency and found that both cold (0°C. to 10°C.) and warmth (35°C. to 45°C.) gave lower fusion points than did intermediate temperatures (23°C. to 33°C.). This would mean that at high and low temperatures the sensitivity of the skin decreases, whereas for a middle range the sensitivity is somewhat higher. It would seem that this work is in harmony with the present investigation although the absolute temperatures involved would be at variance.

From the findings of other investigators and the results of the present study, one has a group of related data. There is the temperature function of vibratory thresholds; the identical nature of this function and that of pressure sensitivity, with the minimal points so very close, seems to leave little doubt that the two processes are intimately related. It should be noted that this optimal temperature, 36.9°C., is very close to normal body temperature, which may be of some significance. Then, too, there is the fact that one is able to evoke vibratory sensations by stimulating with a sub-threshold vibratory stimulus and warming a region into which these disturbances travel. The most interesting feature of this particular experiment is that the vibratory sensations are localized, not at the stimulus needle, but in the warmed area. Further, there seems to be reasonable evidence that the temperature function is approximately the same regardless of the stimulus frequency used.

Theoretical

It was earlier hypothesized that one should expect a drop in intensity thresholds for vibratory sensations with increasing warmth since increasing temperature would cause the chemically active substance to unite or decompose more easily and there should also be a greater volocity of this reaction, since, as has been shown by Arrhenius over half a century ago, the volocity of chemical reactions increases with increasing heat.

If one now assumes that the chemically active substance is present in the end-organ and needs a certain amount of mechanical stimulation to start the reaction, from the above hypothesis it would appear that as temperature increased, the amount of mechanical stimulation necessary (vibratory amplitude in this case) would decrease. However, from the findings of this experiment one would have to make further assumptions to take care of the facts. The above postulations would seem tenable for the first part of the curve, that is, up to the point where with increasing temperature there is a decrease in

thresholds. However, beyond this point one finds that with increased temperature there is an increase in thresholds or sensitivity decreases.

There would seem to be several explanations of this in the light of chemical theory. First, there is a possibility of the effect stated by Millard (11, p. 379) being in operation. "Many biological reactions have a maximum velocity at some characteristic temperature, but the decreased volocity at higher temperature may be due to the destruction of one or more of the reacting substances." If applicable here this would mean that the terminal rise was due to increased temperature making one of the substances less active and thus requiring a greater intensity of stimulation to set off the reaction. This situation is very common in enzymatic action and since enzymes are protein-like substances the analogy may not be so remote as one would imagine at first glance.

Another possibility is the assumption of a catalytic process being involved. If this were the case a rise in temperature might cause a partial destruction of the catalyst; thus a greater intensity of stimulation would be required for the onset of the reaction. This phenomenon of partially destroying a catalyst is especially true of the colloidal type since heat would cause coagulation which would result in this partial destruction.

The majority of the cutaneous end-organs have, in large part, a protein content, and since proteins have a great many colloidal properties the second suggestion might be considered the more reasonable.

A third postulation would be that there is never a decrease in the velocity of the reaction with an increase in temperature, but it continues to increase as temperature is raised. Now, if there were a substance AB which when heated broke down to $A + B$, and A is the activating substance, while B is the substance which causes fatigue or adaptation, then with increasing velocity of the reaction both A and B would be produced at a greater rate. If one then assumes that a certain concentration of A is necessary to initiate an impulse in the absence of B or in the presence of small quantities of this latter substance, then a greater concentration of A would be necessary in the presence of greater quantities of B. If the increase in the production of A were arithmetic then the relative proportion would be changed. For example, if at a given temperature, when the reaction was set off, 10 units of A were produced and 1 unit of B, then the ratio of A to B would be 10 : 1, however, if there were with increasing velocity an increase in A of 8 units, making it now 18, and a similar increase in B, bringing it to 9 units in all, the ratio would no longer be 10 : 1 but 18 : 9 or 2 : 1. Hence a stronger stimulation would be necessary to overcome the effects of the B substance.

It is true that these are not the only possible explanations of the results nor are they true explanations but merely theoretical assumptions which seem tenable. Others may be made, such as that the increase in temperature changes the permeability of the membrane in the end-organ, or changes in pH occur with changes in temperature. However, it would seem that the above three are the most plausible and more general situations from which more specific examples might be derived.

In order to determine the true mechanisms responsible for the results here obtained much more research will be necessary. Such things as the effect of temperature on adaptation time, the effect of temperature on recovery from local anesthesia, the effect of changing pH of a local area of the skin, the frequency function as determined under different conditions of skin temperature, and many other problems will all have to be carefully investigated before any satisfactory theory can be presented.

Summary and Conclusions

1. With increasing skin temperature, vibratory thresholds decrease to a minimum, at which point, with further heating, these thresholds rise.

2. The function uniting temperature and vibratory sensitivity is similar to that linking temperature and pressure sensitivity, with the minimal thresholds falling at approximately the same point.

3. The similarity of the effect of temperature changes on pressure and vibratory stimulation was assumed to be further evidence of the intimacy if not the identity of the mechanisms mediating these two sensory experiences.

4. With decreasing skin temperature vibratory thresholds show a continuous rise.

5. The effects of warming and cooling the skin are shown to be not due to changes in skin conductivity; it is further shown that these changes are not a result of gross vascular action.

6. There is no dependence of the temperature function on stimulus frequency.

7. A chemical theory is thought necessary to explain the results.

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References

1. Allers, R., and Halpern, F., Wechselwirkungen gleichzeitiger Erregung mehrerer Hautsinne. I. Mitteilung. Die Beeinflussung der Tastschwelle durch die Hauttemperatur, *Pflüg. Arch. ges. Physiol.*, 1921, **193**, 595–609.

2. Bellows, R. M., An experimental isolation of some factors determining the response to rhythmic cutaneous stimulation. II. Temperature, *J. exp. Psychol.*, 1937, **20**, 169–177.

3. Brown-Séquard, C. E., *Leçons sur les nerfs vascomoteurs*, Paris: Masson, 1872, pp. 211.
4. Fischer, H., Ueber den Einfluss der Hauttemperatur auf die Prickelempfindung, *Z. Sinnesphysiol.*, 1937, **67**, 149–158.
5. von Frey, M., Das Vibrationsgefühl, eine Leistung des Drucksinns der Haut, *Sitzungsb. phys.-med. Gesellsch. Wurzb.*, 1915, 38–42.
6. Geldard, F. A., The perception of mechanical vibration. I. History of a controversy, *J. gen. Psychol.*, 1940, **22**, 243–269.
7. ——, The perception of mechanical vibration. II. The response of pressure receptors, *J. gen. Psychol.*, 1940, **22**, 271–280.
8. Gilmer, B. v. H., The relation of vibratory sensitivity to pressure, *J. exp. Psychol.*, 1937, **21**, 456–463.
9. Hardy, J. D., and Oppel, T. W., Studies in temperature sensation. IV. The stimulation of cold sensation by radiation, *J. clin. Invest.*, 1938, **17**, 771–778.
10. Kampik, A., Experimentelle Untersuchungen über die praktische Leistungsfähigkeit der Vibrationsempfindungen, *Arch. Psychol.*, 1930, **76**, 3–70.
11. Millard, E. B., *Physical chemistry for colleges*, New York: McGraw-Hill, 1936, pp. 524.
12. Moore, T. V., The influence of temperature and electric current on the sensibility of the skin, *Psychol. Rev.*, 1910, **17**, 347–380.
13. Wagoner, K. S., The effect of warmth and cold stimulation of one hand upon the skin temperature of the contralateral hand, *J. Psychol.*, 1936, **2**, 451–459.
14. —— and Havemann, E. C., The effect of thermal stimulation of one hand upon the temperature limen in the contralateral hand, *J. Psychol.*, 1936, **2**, 441–449.
15. Wedell, C. H., and Cummings, S. B., Fatigue of the vibratory sense, *J. exp. Psychol.*, 1938, **22**, 429–438.
16. Weitz, J., Vibratory sensitivity as affected by local anesthesia, *J. exp. Psychol.*, 1939, **25**, 48–64.

NEUROPHYSIOLOGICAL AND PSYCHOLOGICAL CHANGES INDUCED BY CERTAIN DRUGS: II. ELECTROCORTICAL CHANGES[1]

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Electrocortical potentials seem to offer the possibility of more direct access to the neurophysiological processes underlying behavioral changes than do purely peripheral measurements. Since drugs may be used to produce modifications both in physiological processes and in psychological ones, and since much previous research has been aimed at correlating electroencephalographic changes with changes in these two areas, studies of the electrocortical record in relation to the effects of drugs would seem to afford opportunity for gaining information as to the nature of psychophysiological processes.

Previous investigations concerning the relationship of drug action and electrocortical variability have indicated that this is a valid field of research. Berger (**9**) reported that caffeine, cocaine, atropine and morphine produced a change in the electroencephalographic record characterized by the displacement of the 10/sec. alpha rhythm by a faster beta rhythm.

Gibbs, Gibbs and Lennox (**35**) have studied the action of twenty drugs upon the electrocortical potentials. The only drugs which produced any marked change in the electrocortical record were scopolamine, morphine, ether, alcohol, and a number of the barbiturates. Those drugs which produced muscular symptoms, notably scopolamine, were characterized by high amplitude bursts which correlated with muscular twitches. Drugs such as ether, alcohol, and the barbiturates, when used in anæsthetic doses sufficient to produce stuporous or sleeplike states, gave records showing large amplitude, slow frequency waves, similar to the type encountered in normal sleep.

These studies have not attempted to correlate changes in psychological processes with changes in the electrocortical record. Since the electroencephalogram may serve as a measure of the physiological

[1] This is the second of two papers which together comprise the main content of a dissertation submitted in partial fulfillment of the requirements for the degree of Doctor of Philosophy in the Department of Psychology, in the Graduate College of the State University of Iowa, January, 1940.

[2] Appreciation is expressed to Dr. John R. Knott, of the Department of Psychology, and Dr. Walker Thompson, of the staff of the Psychopathic Hospital of the University of Iowa, for their invaluable assistance. The author is especially indebted to Dr. Knott, who directed this research.

limits of behavior, it is important to know whether or not increase or decrease in its predominate rhythms is related to increase or decrease in the complexity of psychological processes. Utilizing wide empirical limits of frequency, Gibbs (**33**) has suggested that certain types of abnormality are characterized either by a release of energy at extremely slow frequencies, or by a release of energy at extremely fast frequencies. These limits clearly exist in the case of the epilepsies, and there is some reason to believe that a lesser degree of relationship holds in the range of normal behavior, that is, that more rapid frequencies are related to more complex stimulation, as studies such as those of Knott (**46**) and Travis and Egan (**80**) indicate. Upon such evidence, it was decided to attempt to systematically fit the data obtained into the concept of frequency limits of behavior.

Apparatus and Procedure

The same drugs were used as in the experiments upon electrical stimulation, the only difference in the experimental situation being that instead of the electrical measurements, a continuous record of electrocortical potentials was taken.

Contact electrodes of the flat metallic type were placed upon the subject, the indifferent electrode upon the ear lobe and the recording electrode upon the scalp over the left occiput; both were secured with collodion. The apparatus consisted of a four-stage capacitance coupled amplifier (time constant = .67 sec.) coupled to an ink-writing oscillograph by means of a special four-tube matching amplifier.

With the subject in a reclining position, having been instructed to relax and to keep his eyes closed, a control record of several minutes was made. Throughout the recording, obvious movements which might produce muscular artifacts were indicated upon the record, and these portions were disregarded in analyzing the data. All observable physiological and psychological changes were recorded, any marked changes being noted also upon the electroencephalographic record.

An analysis of the records thus obtained was made in terms of the *mean number of waves per second.* Two thirty-second strips of the control portion of the record were read. Two similar strips of the record after the administration of the drug were read, one strip immediately following the injection of the drug, allowing sufficient time for physiological changes to occur, and the other strip several minutes later. These times were approximately 2 to 5 minutes and 25 to 30 minutes after the injection. The mean frequency of the control strips was then compared with the mean frequencies after the injection of the drug. Critical ratios were calculated for differences in mean wave frequency between the control portion of the record and the strips immediately following the injection and later.

Records were made with control injections of physiological saline solution and with the same dosages of drugs as used in the study upon electrical stimulation, with the exception that administration of sodium amytal was discontinued before the full dose was given in cases in which the subject showed signs of drowsiness during the injection.

Subjects.—The subjects used were patients of the Psychopathic Hospital of the University of Iowa and students of the University. Nine were normal subjects, six patients were diagnosed as schizophrenic, four as manic-depressive manic, one as manic-depressive depressed, one as psychoneurotic and two as undiagnosed psychosis.

Results: The Effect of Adrenalin, Mecholyl, Cocaine and Sodium Amytal upon the EEG

Table 1 shows the mean frequencies obtained before and after saline injections administered to five subjects of the types indicated. No psychological changes were indicated except the report of the

TABLE 1

THE EFFECT OF SALINE INJECTIONS UPON ELECTROCORTICAL FREQUENCY (5 CASES)

Type of Case	Mean * F. Control	Mean F. Immediate	Mean F. Later	CR_I	CR_{II}	Change if Significant
Schizophrenia (simple)	11.5 (1.39)	10.7 (1.40)	11.0 (1.14)	2.6	1.8	0
Schizophrenia (simple)	11.5 (1.56)	11.3 (1.29)	10.6 (1.52)	.65	2.62	0
Manic-Depressive (manic)	11.0 (1.28)	11.1 (1.19)	11.1 (1.08)	.37	.37	0
Manic-Depressive (depressed)	11.8 (1.60)	11.0 (1.87)	11.4 (1.70)	2.00	1.07	0
Undiagnosed psychosis	11.1 (1.35)	11.0 (1.10)	10.5 (1.01)	.38	2.37	0

* In this and the following tables, $N = 60$; S.D.'s are indicated in parentheses below the figures for the means.

depressed patient, who said that she felt a little more excited after the injection. The critical ratios indicate that no significant changes in mean wave frequency were obtained in any of these cases.

TABLE 2

THE EFFECT OF ADRENALIN UPON ELECTROCORTICAL FREQUENCY (7 CASES)

Type of Case	Mean F. Control	Mean F. Immediate	Mean F. Later	CR_I	CR_{II}	Change if Significant
Schizophrenia (simple)	12.3 (1.62)	12.1 (1.82)	12.0 (1.60)	.51	.84	0
Manic-Depressive (manic)	10.7 (1.40)	10.5 (1.86)	11.0 (1.17)	.52	1.07	0
Manic-Depressive (depressed)	11.1 (1.38)	11.8 (1.42)	11.2 (1.27)	2.26	.34	0
Undiagnosed psychosis	11.0 (.89)	11.0 (.73)	10.6 (1.12)	0.0	1.72	0
Psychoneurosis (anxiety)	11.6 (1.89)	10.4 (1.05)	11.2 (1.14)	3.87	1.26	—
Normal	10.1 (1.95)	10.4 (1.11)	10.4 (1.38)	.93	.93	0
Normal	10.8 (1.09)	10.5 (2.03)	10.1 (1.53)	.76	2.24	0

Table 2 presents the mean frequencies obtained before and after adrenalin injections given to seven subjects. The only significant change in frequency occurred in the case of the psychoneurotic, who

had been suffering from anxiety symptoms, and who reported a return of these symptoms. In this case there was a statistically significant decrease in mean wave frequency as an immediate effect, the drop being from 11.6 to 10.4 waves per second.

TABLE 3

THE EFFECT OF MECHOLYL UPON ELECTROCORTICAL FREQUENCY (7 CASES)

Type of Case	Mean F. Control	Mean F. Immediate	Mean F. Later	CR_I	CR_{II}	Change if Significant
Schizophrenia (simple)	10.8 (.93)	12.2 (1.60)	11.1 (.88)	4.43	1.49	+
Manic-Depressive (manic)	11.3 (1.42)	11.9 (1.56)	10.9 (1.46)	1.76	1.24	o
Manic-Depressive (depressed)	12.7 (1.79)	14.2 (2.36)	12.4 (1.22)	3.07	.94	+
Undiagnosed psychosis	10.5 (1.12)	10.3 (1.15)	10.3 (1.05)	.79	.83	o
Psychoneurosis	10.9 (1.55)	12,3 (1.91)	12.5 (1.92)	3.48	3.97	+
Normal	11.3 (1.65)	11.0 (1.67)	10.2 (1.79)	.81	2.82	o
Normal	8.2 (1.21)	8.8 (1.83)	7.7 (1.27)	1.72	1.69	o

Table 3 shows the effects of mecholyl injections. In three of the seven subjects, significant increases in mean frequency were obtained shortly following the injection. Since the drug produces marked physiological changes which in most instances cause some discomfort, it is difficult to evaluate psychological changes. The statements of the subjects were all referable to the physiological changes.

Table 4 indicates the effects of cocaine injections. Of the eight subjects, three showed significant increases in mean wave frequency and one showed a significant decrease. These changes all occurred as later effects of the drug, in contrast to the immediate effects obtained with mecholyl. The most marked psychological reaction accompanied the decrease of frequency in the manic patient, who showed an even greater amount of activity, with, however, little attention to environmental stimuli. Another manic patient in this group showed a significant increase in mean wave frequency; psychologically the change was not as marked. He talked more loudly, was somewhat more restless than before the injections and reported a sensation of lightness. The other subjects with whom significant increases in frequency were found were normal individuals, one of

whom showed a marked psychological reaction, characterized by extreme anxiety. The other normal subject reported vivid mental images and a return of his faculty of eidetic imagery which he had seen failing for some time.

TABLE 4

THE EFFECT OF COCAINE UPON ELECTROCORTICAL FREQUENCY (8 CASES)

Type of Case	Mean F. Control	Mean F. Immediate	Mean F. Later	CR_I	CR_{II}	Change if Significant
Schizophrenia (paranoid)	7.90 (2.33)	8.00 (1.52)	8.80 (1.73)	.25	2.53	o
Schizophrenia (paranoid)	10.8 (1.40)	10.9 (1.22)	11.1 (1.43)	.35	.94	o
Manic-Depressive (manic)	10.3 (1.20)	10.1 (1.76)	8.10 (1.30)	.56	7.78	−
Manic-Depressive (manic)	10.9 (1.56)	11.0 (1.82)	14.7 (3.27)	.26	6.02	+
Manic-Depressive (manic)	10.8 (1.07)	10.6 (1.40)	11.2 (1.67)	.69	1.19	o
Manic-Depressive (depressed)	10.1 (1.42)	10.1 (1.19)	10.5 (1.23)	0.0	1.38	o
Normal	9.12 (1.67)	8.47 (1.61)	10.4 (1.76)	1.79	3.30	+
Normal	10.2 (.92)	10.7 (.62)	11.0 (.79)	3.05	4.30	+

Table 5 presents the values obtained in the experiments with sodium amytal. In all of these cases except one a significant increase in wave frequency was found. Of the ten cases showing this effect, eight showed an immediate significant change, while in the other two the change was significant only in the later reading; however, even in these two cases the immediate mean wave frequency is higher than that of the control. In all of these cases the psychological effect of the drug was quite noticeable, in contrast to the inconsistency of reaction to the other drugs. Six of the ten subjects showed increase in speech activity, while a heightened affective state, tending towards euphoria, was observed in all but two. The reaction of the depressed patient who showed an increase in mean wave frequency which is not statistically significant was not essentially different from the others.

DISCUSSION

1. *The Effects of Adrenalin*

The results of the experiments with adrenalin are essentially the same as those reported in the electroencephalographic study by Gibbs,

TABLE 5

THE EFFECT OF SODIUM AMYTAL UPON ELECTROCORTICAL FREQUENCY (11 CASES)

Type of Case	Mean F. Control	Mean F. Immediate	Mean F. Later	CR_I	CR_{II}	Change if Significant
Schizophrenia (paranoid)	13.0 (2.40)	14.8 (2.38)	14.9 (1.83)	3.38	4.18	+
Schizophrenia (catatonic)	9.40 (1.77)	13.4 (1.85)	11.7 (2.05)	9.83	5.23	+
Schizophrenia (catatonic)	10.9 (1.40)	13.0 (1.85)	12.7 (1.68)	5.46	5.04	+
Schizophrenia (catatonic)	11.6 (2.09)	15.8 (1.83)	11.2 (1.83)	9.78	.94	+
Manic-Depressive (manic)	11.0 (1.94)	14.3 (2.08)	13.2 (2.21)	7.25	3.98	+
Manic-Depressive (manic)	10.0 (1.75)	10.8 (1.63)	11.7 (1.97)	2.14	4.01	+
Manic-Depressive (depressed)	13.0 (2.08)	13.9 (1.87)	13.1 (2.82)	2.07	.17	o
Undiagnosed psychosis	12.2 (2.24)	13.3 (1.85)	14.0 (1.70)	2.14	3.62	+
Normal	9.90 (.85)	10.7 (1.33)	9.93 (.85)	3.01	.16	+
Normal	10.4 (1.26)	13.1 (1.29)	10.6 (1.91)	9.51	.52	+
Normal	9.77 (1.06)	10.5 (1.09)	11.1 (1.29)	3.03	4.87	+

Gibbs and Lennox (**35**), who obtained no appreciable effect upon the electrocortical record in six experiments in which doses of 0.5–0.8 mg. of epinephrine hydrochloride were given. In the one case in the present study in which a significant change was found, *i.e.*, that of the psychoneurotic who reported a return of his anxiety symptoms, the drop in mean frequency is possibly related to the changes noted by Lemere (**60**) and others with respect to the emotional state of the subject. Rubin, Hoagland and Cameron found an increase in the delta index (basically a frequency measure) accompanying states of emotion. Lemere reports a decrease in the amount of alpha rhythm present under conditions of emotional tension. Berger (**6**) and Adrian (**7**) have also commented upon the necessity of having the subject under optimum conditions of relaxation in order to obtain the maximum amount of alpha activity, other factors being equal; and it may be noted parenthetically that muscular tension may appear as a specific component of what is referred to as 'emotional

tension,' since, according to Cannon's viewpoint, an emotional state acts to prepare the organism for immediate action. Therefore, if it is borne in mind that the measure taken is the *mean number of waves per second*, it is not surprising that a lower value was obtained after the injection of the drug, with its concomitant psychological and physiological changes.

2. *The Effects of Mecholyl*

As previously noted, the physiological changes produced by this drug tend to render difficult any evaluation of psychological changes *per se*, and for this reason no attempt to correlate behavioral changes with changes in the electroencephalogram is made. However, a significant increase in mean wave frequency was found in a portion of these cases, and a tentative explanation for the changes thus produced may be advanced.

Mecholyl is acetyl-beta-methylcholine, a choline derivative which differs from certain other choline derivatives, according to Simonart (**77**), in that it has a powerful stimulating effect upon the terminations of the post-ganglionic fibers of the parasympathetic nerves, while the nicotine-like effect of choline, acetylcholine and other derivatives upon the autonomic ganglia is feeble or absent. It is said to be more stable than acetylcholine and less easily hydrolyzable by the tissues.

In the mecholyl series of experiments, the typical effects of parasympathetic stimulation were elicited, to a greater or lesser degree, depending upon the individual subject. Of these effects, the ones to be considered here are vasodilatation, as evidenced peripherally by flushing of the skin, constriction of the bronchioles, as indicated by labored respiration and reported difficulty in breathing, and a tendency to lowered systemic arterial blood pressure. There was also tendency toward increase in pulse rate with the dosages given.

Various workers have investigated the changes in the electroencephalographic record in response to cerebral circulatory changes and to variation in the alveolar tensions of carbon dioxide and oxygen. Bremer (**13**) has reported that the presence of 20 percent CO_2 in respired air caused the disappearance of slow rhythms with remaining low amplitude, higher frequency potentials, as recorded from the 'isolated' cortex of the cat. Lennox, Gibbs and Gibbs (**63**) have shown that the rate of cortical potentials in man is influenced by changes in CO_2. Gerard (**30, 32**) has shown similar effects in man, reporting increased rhythms as the effect of CO_2 lack, and increased rate and amplitude, followed by abolition of the rhythm, with CO_2 in great excess. With mild CO_2 excess, as in voluntary apnea, he reports that the 10/sec. waves are broken up into more rapid wavelets, while the rapid 40/sec. rhythm is probably enhanced. Sugar and

Gerard (78) report that during advancing anoxia, brain potentials pass through a phase of increased frequency and often increased amplitude, indicating a transient stimulation en route to depression. Gibbs, Davis and Lennox (34) obtained slower waves of cortical potentials when an acute fall in blood pressure was produced by pressure stimulation in the case of a subject possessing a hyperactive carotid sinus reflex. Similar dependence of the characteristics of the electrocortical record upon oxygen tension has been shown by Davis, Davis and Thompson (22).

Heinbecker (37), Thompson and Kimball (79) and others have shown changes of a comparable nature in nerve fibers, indicating an increase in the irritability of nerve at the onset of asphyxia. Thus there appears to be considerable evidence that interference with the vital gas exchange processes, whether through anoxemia, hypercapnia, or reduction in cerebral blood supply, may result in a period of hyperirritability of the nerve tissue involved.

Possible cerebral circulatory changes need also to be considered in connection with the mecholyl experiments, since, according to McNaughton (71), the innervation of the intracranial vessels does not differ essentially from the innervation of blood vessels elsewhere in the body. Schmidt and Hendrix (74) found evidence of intracranial vasodilation upon injection of acetylcholine and of mecholyl in cats and rabbits. It is also known that many drugs, such as nitrites and alcohol, dilate the cerebral blood vessels (27), and increased CO_2 tension is also said to produce such an effect by direct action upon the walls of the blood vessels (70). Bronk (15), Lennox, Gibbs and Gibbs (62) and others have emphasized the fact that cerebral blood flow is normally controlled by the amount of cerebral activity. However, in the case of a drug which acts directly upon the circulatory system, such as the one under consideration, we might expect to find changes in the electroencephalogram in which vasodilator effects play a part.

The importance of oxygen lack, and, particularly, of carbon dioxide excess, in local vasomotor control of the brain, has been pointed out by Gerard (31). The factors involved, then, in considering vasodilation as a possible mechanism in the present problem are the action of mecholyl and the action of excess CO_2 or of oxygen lack.

In view of the evidence quoted, it seems plausible that the following chain of events is responsible for the increase in mean wave frequency of the electroencephalographic record found in a certain proportion of the cases in which mecholyl was used: (1) Peripheral vasodilation tends to lower the systematic arterial blood pressure, although an increased heart rate may keep the pressure at approximately the same level in some cases. (2) Constriction of the bron-

chioles, due to the action of mecholyl, occurs. Under such conditions respiration is more difficult and the alveolar CO_2 is increased. Simultaneously oxygen lack may occur. (3) Vasodilation of the cerebral blood vessels, due to the action of mecholyl, occurs. (4) The vasodilation of the cerebral blood vessels is augmented by the direct effects upon the vessels of CO_2 excess and possibly oxygen lack. (5) Under these conditions, the CO_2 balance between the cerebral tissues and the blood stream is no longer normal, and removal of this metabolite is not as efficient. (6) The typical alterations in the electroencephalographic record due to CO_2 excess, and possibly oxygen lack, are found.

An alternative explanation might be made, of course, upon the basis that the augmented cortical activity is due, not to the stimulating action of excess CO_2, but to the action of mecholyl upon the synapses, in accordance with the neurohumoral view that acetylcholine acts as a mediating substance in synaptic transmission in the central nervous system. While this is a possibility, the present uncertainty as to the nature of synaptic transmission in the central nervous system is well known. Cannon and Rosenblueth (**17**) have indicated the overwhelming evidence for chemical mediation in the autonomic nervous system, while pointing out that such evidence is of necessity inferential; and the greater difficulty of obtaining even inferential evidence of an unquestioned nature about synaptic transmission in the central nervous system is obvious because of the characteristics of this system. In support of a chemical interpretation of the results, however, is the work of Bonnet and Bremer (**10**), who report a clear stimulating effect upon the 'isolated' cerebral cortex of the cat, as manifested by its electrical activity, after the injection of small dosages of acetylcholine into the carotid artery. Among the effects reported are an augmentation in frequency of the spontaneous waves. Further research should clarify the nature of this effect.

3. *The Effects of Cocaine*

As noted before, there is apparently a correlation between psychological changes and electrocortical changes under the effects of this drug. One case in this group is divergent from the others, that of the manic patient who showed an augmentation of his excited state, accompanied by a significant decrease in mean wave frequency of the electroencephalographic record, in contrast to the other cases in which the psychological changes were accompanied by a significant increase in mean wave frequency. Fischer and Lowenbach (**25**, **26**) report large electrical disturbances of the cortex with stimulating drugs, of which cocaine is included. Berger (**9**) referred to the faster beta rhythm which displaced the alpha rhythm in his experiments

with this drug as indicative of change in the psychic processes. While it is somewhat difficult to order descriptions of behavior given in such terms as restlessness, garrulousness, aggressiveness, impulsiveness, and alertness (**19**, **67**) to a neurophysiological concept such as that of cortical excitatory state as set forth by Jasper (**35**, **40**), there appears to be little doubt that cocaine, in effective doses, has a stimulating effect upon the central nervous system, and that this effect spreads from the higher centers to the lower ones as the size of the dosage is increased (**19**). The results of the present series of cocaine experiments, then, seem to be in accord with the general view that increased frequency of waves in the electrocortical record is indicative of augmented cortical activity (**41**).

4. *The Effects of Sodium Amytal*

The most consistent electrocortical changes were obtained in the series of experiments with this drug, and psychological changes were obtained in each case. Better control over the effects of the drug is to be attributed in this case to the method of administration, which was by intravenous injection, thus allowing an observance of immediate effects while the drug was being given. The psychological changes vary individually, and are dependent upon the dosage and rate of administration, but in general there is a tendency toward euphoria, loosening up of speech, loss of some psychological 'inhibitions,' and general bodily relaxation, followed by drowsiness or sleep if the subject is not stimulated or if the dosage is carried further. An attempt was made in this series of experiments to avoid sleep-inducing concentrations of the drug.

Previous research has shown that amytal may increase the amount of alpha rhythm present in the electrocortical record, presumably because of its relaxing effect, just as conditions of relaxation in general tend to favor the production of the alpha rhythm (**1**, **6**). Gibbs, Gibbs and Lennox (**35**), using phenobarbital sodium and pentobarbital sodium given intravenously, report changes in the records comparable to those obtained during normal sleep when dosages sufficient to impair consciousness were given. Bremer (**14**) has presented electroencephographic evidence to show that the effects of the volatile anæsthetics, such as ether, differ in their mode of action from that of the non-volatile anæsthetics, such as the barbiturates. Berger (**7**, **8**) has also commented upon this difference. Beecher, McDonough and Forbes (**39**, **40**) and Forbes and Morison (**28**) have raised this question in connection with their experiments on sensory stimulation under barbiturate anæsthesia. Bremer's view was that ether acts equally on all elements of the sensory response, while the barbiturates act selectively on the after-discharge, which he relates to intercalated

cortical neurones. Of interest in regard to the possibility of thalamic block proposed by Berger (7) is the work of Keeser and Keeser (42, 43), who studied the distribution of barbiturates in the central nervous system and found the greatest concentrations in the diencephalon, especially in the thalamus, while Koppanyi, Dille and Krop (44), using larger dosages, found the distribution equal throughout the various parts of the central nervous system.

Bremer (12) and Derbyshire, Rempel, Forbes and Lambert (23) report the appearance of low amplitude, higher frequency potentials as the immediate effect of ether upon the cat, and Berger (5) obtained an increase in the higher frequency potentials of the human electroencephalogram during the excitation phase of chloroform narcosis. Gibbs, Gibbs and Lennox (35) observed higher frequency waves under alcoholic intoxication. Beecher and McDonough (2), making a statistical study of mean wave frequency, report higher mean frequencies with the volatile anæsthetics than with non-volatile anæsthetics. Control frequencies, however, are not given. There is thus a variety of evidence which would point to differences between the volatile and non-volatile anæsthetics when given in large dosages; however, as Walter (81) points out, the barbiturates when used in smaller dosages do not necessarily have the effects on the electroencephalogram described.

Moreover, if the psychological changes are considered, the tendency to euphoria under sodium amytal in the present experiments and the similar tendency under alcohol, high or low oxygen tension, chloroform and ether is suggestive of a common stimulating effect upon the cortex, either by removal of inhibitory influences, thereby allowing an augmented activity, or to direct effects upon the metabolism of the cortical neurones, such as those postulated by Rubin and Freeman (72) in their study of the influence of cyanide on human brain potentials. Perhaps the only study directly comparable to the present one on sodium amytal is that of Lemere (61), who gave slow intravenous injections of sodium amytal to schizophrenic patients and noted an accentuation of the alpha rhythm during the prenarcotic stage of its action, which he indicates parallels the clinical improvement with increased accessability and rapport. With continued administration of the drug, he reports that the alpha waves break up into faster, more irregular waves, interspersed with delta waves. He suggests, from his work, and that of Shapiro (75) on oxygen consumption rate under sodium amytal, that this drug probably acts by depressing cellular oxidation, with resulting anoxia, and an increase in excitation followed by depression of the cortical neurones.

Summary and Conclusions

Neurophysiological measures and psychological changes have been studied in relation to the action of four drugs, adrenalin, mecholyl, cocaine and sodium amytal, upon normal subjects and mental patients. Control experiments with saline injections were also made.

The experiments have been divided into two groups. In the first group measurements of rheobase, chronaxie, and effects of repeated stimulation upon intensity and duration of the stimulating current with reference to the chronaxie point were made before and during the course of action of the drug. In the second group electrocortical potentials were recorded throughout the experiment. In both groups a record of psychological changes directly observed or reported by the subject was kept.

The electrical measurements made in the first group of experiments did not show any tendency to marked or consistent changes after the administration of any of the drugs. Marked psychological changes were obtained in several of the experiments. Difficulties encountered in clinical measurements of this type of physiological response and controversial theoretical points which might have a bearing upon the results are discussed. It appears probable that a single point on the strength-duration curve cannot adequately characterize excitability. Summation effects in the intensity and time parameters obtained with repeated stimuli do not appear to give further aid in such characterization when the measurements are made with respect to a single point on the curve.

In the second group of experiments, in which electroencephalographic records were secured, a statistical treatment of the data in terms of the mean number of waves per second was made. No statistically significant changes in mean wave frequency were found in the experiments with saline injections. No psychological changes were obtained in this group except a subjective report of slight excitement by one patient.

In the adrenalin experiments, one significant decrease in the mean number of waves was found, this being in the case of a psychoneurotic who reported a return of his anxiety symptoms. No other significant changes were found, nor were there any marked psychological changes in the other experiments with adrenalin in this series.

The series of mecholyl experiments showed significant increases in the mean number of waves per second in three of the seven cases. Evaluation of psychological changes was difficult because of the marked basic physiological reactions which the drug induces. A tentative explanation for the changes obtained in mean wave frequency of the electroencephalogram is advanced.

Of the eight cocaine experiments, three showed significant increases in mean frequency and one a significant decrease in mean wave frequency of the electrocortical record. These changes appear to be related to psychological changes; marked psychological changes did not occur in the experiments in which no significant change in mean number of waves per second was obtained.

In all except one of the experiments with sodium amytal, significant increases in mean frequency were found. The psychological effect of the drug was quite noticeable in all of the cases.

From the evidence reported, it is difficult to state whether or not the psychological and physiological changes accompanying the action of the drugs used are attended by changes in the peripheral nervous system, since failure to obtain indications of such changes by the measurements described (chronaxie) might be due either to the absence of such changes or to the difficulty in detecting them by the methods employed.

There appears to be a fairly consistent relationship between psychological changes and significant changes in the mean number of waves per second of the electroencephalogram, when such changes occur as the effects of the drugs used. Increased 'mental activity' is associated with increased frequency. No data pertaining to a downward direction of change were available in this research. Experiments of this nature would seem to offer a fruitful field for further research concerning physiological correlates of behavior.

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References

1. Adrian, E. D., The electrical activity of the cortex, *Proc. roy. Soc. Med.*, 1935, **29**, 197–200.
2. Beecher, H. K., and McDonough, F. K., Cortical action potentials during anæsthesia, *J. Neurophysiol.*, 1939, **2**, 289–307.
3. Beecher, H. K., McDonough, F. K., and Forbes, A., Effects of blood pressure changes on cortical potentials during anæsthesia, *J. Neurophysiol.*, 1938, **1**, 324–331.
4. Beecher, H. K., McDonough, F. K., and Forbes, A., Similarity of effects of barbiturate anæsthesia and spinal transection, *J. Neurophysiol.*, 1939, **2**, 81–88.
5. Berger, H., Uber das Electrenkephalogram des Menschen. I. Mitteilung, *Arch. Psychiat. Nervenkr.*, 1929, **87**, 527–570.
6. Berger, H., *Ibid.*, II. Mitteilung, *J. Psychol. Neurol., Lpz.*, 1930, **40**, 160–179.
7. Berger, H., *Ibid.*, VIII. Mitteilung, *Arch. Psychiat. Nervenkr.*, 1933, **101**, 452–469.
8. Berger, H., *Ibid.*, IX. Mitteilung, *Arch. Psychiat. Nervenkr.*, 1934, **102**, 538–557.
9. Berger, H., *Ibid.*, XIII. Mitteilung, *Arch. Psychiat. Nervenkr.*, 1937, **106**, 577–584.
10. Bonnet, V., and Bremer, F., Action du potassium, du calcium, et de l'acetylcholine sur les activites electriques spontanees et provoquees, de l'ecorce cerebrale, *C. R. Soc. Biol.*, Paris, 1937, **126**, 1271–1275.
11. Bourguignon, G., *La chronaxie chez l'homme*, Paris: Libraires de l'Academie de Medecine, 1923.
12. Bremer, F., Action du differents narcotiques sur les activites electriques spontanees et reflexes de l'ecorce cerebrale, *C. R. Soc. Biol.*, Paris, 1936, **121**, 861–866.
13. Bremer, F., Action de l'anoxemie de l'hypercapnie et de l'acapnie sur l'activite electrique de l'ecorce cerebrale, *C. R. Soc. Biol.*, Paris, 1936, **123**, 1256–1257.

14. Bremer, F., Difference d'action de la narcose etherique et due sommeil barbiturique sur les reactions sensorielles acoustiques du cortex cerebrale. Signification de cette difference en ce qui concerne le mecanisme du sommeil, *C. R. Soc. Biol.*, Paris, 1937, **124**, 848–852.
15. Bronk, D. W., The influence of circulation on the activity of nerve cells, In *The circulation of the brain and spinal cord*, Baltimore: Williams and Wilkins Company, 1938, pp. 298–315.
16. Buchanan, D. N., and Garven, H. S. D., The chronaxie in tetany. The effect on the chronaxie of thyreoparathyreoidectomy, the administration of guanidine and of di-methyl guanidine, *J. Physiol.*, 1926, **62**, 115–128.
17. Cannon, W. B., and Rosenblueth, A., *Autonomic neuro-effector systems*, New York: The Macmillan Co., 1937.
18. Colle, J., and Delville, P., Les parametres de l'excitabilite, *C. R. Soc. Biol.*, Paris, 1932, **111**, 1072–1075.
19. Cushny, A. R., *A textbook of pharmacology and therapeutics*, Rev. by C. W. Edmunds and J. A. Gunn, Philadelphia: Lea & Febiger, 1936.
20. Davis, H., The relationship of the 'chronaxie' of muscle to the size of the stimulating electrode, *J. Physiol.*, 1923, **57**, lxxxi–lxxxii.
21. Davis, H., and Forbes, A., Chronaxie, *Physiol. Rev.*, 1936, **16**, 407–441.
22. Davis, P. A., Davis, H., and Thompson, J. W., Progressive changes in the human electroencephalogram under low oxygen tension, *Amer. J. Physiol.*, 1938, **123**, 51–52.
23. Derbyshire, A. J., Rempel, B., Forbes, A., and Lambert, E. F., The effects of anæsthetics on action potentials in the cerebral cortex of the cat, *Amer. J. Physiol.*, 1936, **116**, 577–596.
24. Erlanger, J., and Gasser, H. S., *Electrical signs of nervous activity*, Philadelphia: University of Pennsylvania Press, 1937.
25. Fischer, M. H., and Lowenbach, H., Aktionsstrome des Zentralnervensystems unter der Einwirkung von Krampfgiften. I. Mitteilung. Strychnin und Picrotoxin, *Arch. f. Exp. Path.*, 1934, **174**, 357–382.
26. Fischer, M. H., and Lowenbach, H., Aktionsstrome des Zentralnervensystems unter der Einwirkung von Krampfgiften. I. Mitteilung. Strychnin und Picrotoxin, *Arch. f. exper. Path.*, 1934, **174**, 357–382.
27. Forbes, H. S., and Cobb, S., Vasomotor control of cerebral vessels, In *The circulation of the brain and spinal cord*, Baltimore: Williams & Wilkins Company, 1938, pp. 201–217.
28. Forbes, A., and Morison, B. R., Cortical response to sensory stimulation under deep barbiturate narcosis, *J. Neurophysiol.*, 1939, **2**, 112–128.
29. Frederique, H., Testing excitability by means of a time factor, *Physiol. Rev.*, 1928, **8**, 501–544.
30. Gerard, R. W., Factors influencing brain potentials, *Trans. Amer. Neurol. Ass.*, 1936, **62**, 55–60.
31. Gerard, R. W., Brain metabolism and circulation, In *The circulation of the brain and spinal cord*, Baltimore: Williams & Wilkins Company, 1938, pp. 316–345.
32. Gerard, R. W., Factors controlling brain potentials, *Cold Spr. Harb. Sympos. quant. Biol.*, 1936, **4**, 292–304.
33. Gibbs, F. A., Interpretation of the electroencephalogram, *J. Psychol.*, 1937, **4**, 365–382.
34. Gibbs, F. A., Davis H., and Lennox, W. G., The electroencephalogram in epilepsy and in conditions of impaired consciousness, *Arch. Neurol. Psychiat.*, Chicago, 1935, **34**, 1133–1148.
35. Gibbs, F. A., Gibbs, E. L., and Lennox, W. C., Effect on the electroencephalogram of certain drugs which influence nervous activity, *Arch. intern. Med.*, 1937, **60**, 154–166.
36. Grundfest, H., Excitability of the single fibre nerve-muscle complex, *J. Physiol.*, 1932, **76**, 95–115.
37. Heinbecker, P., Effect of anoxemia, carbon dioxide and lactic acid on electrical phenomena of myelinated fibers of the peripheral nervous system, *Amer. J. Physiol.*, 1929, **89**, 58–83.
38. Jasper, H. H., A laboratory study of diagnostic indices of bilateral neuromuscular organization in stutterers and normal speakers, *Psychol. Monogr.*, 1932, **43**, 72–174.
39. Jasper, H. H., Cortical excitatory state and synchronism in the control of bioelectric autonomous rhythms, *Cold Spr. Harb. Sympos. quant. Biol.*, 1936, **4**, 320–338.
40. Jasper, H. H., Cortical excitatory state and variability in human brain rhythms, *Science*, 1936, **83**, 259–260.
41. Jasper, H. H., Electrical signs of cortical activity, *Psychol. Bull.*, 1937, **34**, 411–481.
42. Keeser, E., and Keeser, I., Uber den Nachweis von Caffein, Morphin und Barbitursaurederivaten im Gehirn, *Arch. f. exp. Path. u. Pharm.*, 1928, **127**, 230–235.

43. KEESER, E., AND KEESER, I., Studies on barbiturates. Distribution of barbiturates in the brain. Remarks on the publication of the same title by Koppanyi, Dille and Krop, *J. Pharm. exp. Ther.*, 1935, **53**, 137.
44. KOPPANYI, T., DILLE, J. M., AND KROP, S., Studies on barbiturates. VIII. Distribution of barbiturates in the brain, *J. Pharmacol. exp. Ther.*, 1934, **52**, 121–128.
45. KNOEFEL, P. K., Narcosis and the chronaxie, *J. Pharmacol. exp. Therap.*, 1935, **55**, 72–81.
46. KNOTT, J. R., Brain potentials during silent and oral reading, *J. gen. Psychol.*, 1938, **18**, 57–62.
47. LAMBERT, E. F., SKINNER, B. F., AND FORBES, A., Some conditions affecting intensity and duration thresholds in motor nerve, with reference to chronaxie of subordination, *Amer. J. Physiol.*, 1933, **106**, 721–737.
48. LANDIS, C., AND HUNT, W. A., Adrenalin and emotion, *Psychol. Rev.*, 1932, **39**, 467–485.
49. LAPICQUE, L., Definition experimentale de l'excitabilite, *C. R. Soc. Biol.*, Paris, 1909, **67**, 280–283.
50. LAPICQUE, L., *L'excitabilite en fonction du temps*, Paris: Les Presses Universitaires de France, 1926.
51. LAPICQUE, L. AND M., La chronaxie de subordination, sa regulation reflexe, *C. R. Soc. Biol.*, 1928, **99**, 1947–1949.
52. LAPICQUE, L., The chronaxic switching in the nervous system, *Science*, 1929, **70**, 151–154.
53. LAPICQUE, L., On electrical stimulation of muscle through Ringer's solution, *J. Physiol.*, 1931, **73**, 219–246.
54. LAPICQUE, L., GASSER, H. S., AND DESOILLE, A., Relation entre le degree d'heterogeneite des nerfs et la complexite de leur courant d'action, *C. R. Soc. Biol.*, Paris, 1925, **22**, 9–10.
55. LAPICQUE, L., AND LEGENDRE, R., Alterations des fibres nerveuses myeliniques sous l'action des anesthesiques et de divers poisons nerveux, *J. de Physiol. et de Path. Gen.*, 1922, **20**, 163–172.
56. LAPICQUE, M., Role des centres dans l'action peripherique de la strychnine, *C. R. Soc. Biol.*, Paris, 1933, **111**, 957–959.
57. LAPICQUE, M., AND NATTAN-LARRIER, M., Action de l'adrenaline sur l'excitabilite musculaire et sur la fatigue, *C. R. Soc. Biol.*, Paris, 1922, **86**, 474–476.
58. LAPICQUE, M., AND VAHL, F., Influence des divers centres nerveux sur les chronaxies peripheriques sensitive et motrice. Action de la cafeine, *C. R. Soc. Biol.*, Paris, 1931, **108**, 1136–1138.
59. LAST, S. J., AND STROM-OLSEN, R., Chronaximetric studies in catatonia, *J. ment. Sci.*, 1936, **82**, 763–772.
60. LEMERE, F., The significance of individual differences in the Berger rhythm, *Brain*, 1936, **59**, 366–375.
61. LEMERE, F., Effects on the electroencephalogram of various agents used in treating schizophrenia, *J. Neurophysiol.*, 1938, **1**, 590–595.
62. LENNOX, W. G., GIBBS, F. A., AND GIBBS, E. L., The relationship in man of cerebral activity to blood flow and to blood constituents, In *The circulation of the brain and spinal cord*, Baltimore: Williams & Wilkins Company, 1938, pp. 277–297.
63. LENNOX, W. G., GIBBS, F. A., AND GIBBS, E. L., Effect on the electroencephalogram of drugs and conditions which influence seizures, *Arch. Neurol. Psychiat.*, Chicago, 1936, **36**, 1236–1250.
64. LINDEMANN, E., The neurophysiological effect of intoxicating drugs, *Amer. J. Psychiat.*, 1934, **13**, 1007–1037.
65. LINDEMANN, E., The psychopathological effect of drugs affecting the vegetative system. I. Adrenalin, *Amer. J. Psychiat.*, 1935, **91**, 983–1057.
66. LINDEMANN, E., AND FINESINGER, J. E., The effect of adrenalin and mecholyl in states of anxiety in psychoneurotic patients, *Amer. J. Psychiat.*, 1938, **95**, 353–370.
67. LINDEMANN, E., AND MALAMUD, W., Experimental analysis of the psychopathological effects of intoxicating drugs, *Amer. J. Psychiat.*, 1934, **13**, 853–879.
68. LUCAS, K., On the rate of variation of the exciting current as a factor in electric excitation, *J. Physiol.*, 1907, **36**, 253–274.
69. MALAMUD, W., LINDEMANN, E., AND JASPER, H. H., Effects of alcohol on the chronaxia of the motor system, *Arch. Neurol. Psychiat.*, Chicago, 1933, **29**, 790–807.
70. *McLeod's physiology in modern medicine* (Ed. by Philip Bard), 8th ed., St. Louis: C. V. Mosby Company, 1938.

71. McNaughton, F. L., The innervation of the intracranial blood vessels and dural sinuses, In *The circulation of the brain and spinal cord*, Baltimore: Williams & Wilkins Company, 1938, pp. 178–200.
72. Rubin, M. A., and Freeman, H., The influence of cyanide on brain potentials in man, *J. Neurophysiol.*, 1938, **1**, 527–532.
73. Rudeanu, A., and Bonvallet, M., Action de la scopolamine sur les chronaxies motrices peripheriques. Relation avec la coordination des mouvements, *C. R. Soc. Biol.*, Paris, 1932, **109**, 1193–1195.
74. Schmidt, C. F., and Hendrix, J. P., The action of chemical substances on cerebral blood vessels, In *The circulation of the brain and spinal cord*, Baltimore: Williams & Wilkins Company, 1938, pp. 229–276.
75. Shapiro, L. B., Sodium amytal: effects on oxygen consumption rate in the psychoses, *J. nerv. ment. Dis.*, 1937, **85**, 305–309.
76. Shock, N. W., Some psychophysiological relations, *Psychol. Bull.*, 1939, **36**, 447–476.
77. Simonart, A., On the true methyl cholines, *J. Pharmacol. exp. Ther.*, 1935, **54**, 105–130.
78. Sugar, O., and Gerard, R. W., Anoxia and brain potentials, *J. Neurophysiol.*, 1938, **1**, 558–572.
79. Thompson, I. M., and Kimball, H. S., Effects of local ischemia upon human nerve fibers in vivo, *Proc. Soc. exp. Biol. Med.*, 1936, **34**, 601–603.
80. Travis, L. E., and Egan, J. P., Increase in frequency of the alpha rhythm by verbal stimulation, *J. exp. Psychol.*, 1938, **23**, 384–393.
81. Walter, W. G., Critical review: the technique and application of electroencephalography, *J. Neurol. Psychiat.*, Chicago, 1938, **1**, 359–385.
82. Wechsler, D., and Freeman, R. G., Studies in chronaxie. I. Methodology and normal variations, with a report of thirty cases of children, *Arch. Neurol. Psychiat.*, Chicago, 1929, **22**, 558–569.
83. Weiss, G., La loi de l'excitation electrique des nerfs, *C. R. Soc. Biol.*, Paris, 1901, **53**, 466–488.
84. Weisz, S., Malamud, W., and Fowler, O. D., The effect of repeated stimuli upon chronaxia in normal and mentally diseased persons, *Trans. Amer. Neurol. Ass.*, 1937, **63**, 131–134.
85. Wilson, M. O., Chronaxie, *Psychol. Bull.*, 1935, **32**, 4–33.

SOME RELATIONSHIPS BETWEEN ELECTRICAL SIGNS OF CENTRAL AND PERIPHERAL ACTIVITY: II. DURING 'MENTAL WORK' [1, 2]

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It has been previously indicated (**9**) that the electrical signs of cortical, cardiac and muscular activity appear to be unrelated variables during a condition of 'rest.' This general statement apparently holds when 'rest' is defined as obtaining whenever the muscle action potential is below 13 microvolts. There was an indication that there might be positive relationships between these electrical signs when the muscle action potential amplitude was above 13 microvolts.

A considerable body of evidence has been produced which indicates that a positive relationship holds between the amount of 'psychological work' and the amplitude of the muscle action potential. Such 'psychological work' as the memorizing of meaningful material (**3**), the memorizing of nonsense syllables (**5**), 'mental' arithmetical computations (**3, 4**), and maze learning (**2**) has been fitted into the basic equation, Δ Action Potential $= f$ ('Work').

Workers in the field of electroencephalography have noted that 'mental work' and 'attention' tended to disrupt the alpha rhythm of the record (**8**). Later studies have demonstrated quantitative variations in the electroencephalogram which can be expressed in terms of frequency and *apparently* are a function of 'work.' Reading, silently and aloud (Knott, **11**), listening to meaningful speech (Travis and Egan, **15**) and speaking (Travis and Knott, **16**) have been correlated with increments in the mean frequency of the electroencephalogram. 'Mental multiplication' has qualitatively been observed to correlate with 'beta rhythm' rather than with 'alpha rhythm' activity (frequencies of about 20 rather than 10 per second) (**1**). Whether or not these data can be fitted into a general formulation in terms of Gibbs' frequency-spectrum concept (**7**) is not yet known, although Knott (**12**) has made a preliminary generalization which has yet to be subjected to rigorous test.

Two such discrete sets of data, coupled with the data on the

[1] This study is the second of a series of papers bearing upon this general topic under the direction of Dr. John R. Knott in the laboratories of the department of Psychology in the State University of Iowa.

[2] The main content of Part II of a dissertation submitted in partial fulfillment of the requirements for the Degree of Doctor of Philosophy in the Department of Psychology in the Graduate College of the University of Iowa.

[3] The author wishes to express his gratitude to Dr. John R. Knott, who directed this research.

relationship between electroencephalographic and electromyographic variations during 'rest,' make increasingly necessary the simultaneous recording of these peripheral and central indices of activity and an evaluation of the results with reference to physiological theories of psychological processes.

Problem

Specifically, the problem of the present investigation was to ascertain the degree of relationship between variations in electroencephalographic frequency, electrocardiographic frequency, and electromyographic amplitude during increasingly difficult 'mental' multiplications.

Apparatus and Procedure

The recording technique and the apparatus used were the same as previously described (9).

The experimental procedure was as follows: The subjects reclined comfortably upon a daybed in a darkened, electrostatically shielded room and were told that they were to be given multiplication problems to 'work in their heads.' They were cautioned not to use their fingers as aids in calculating and to remain as comfortable as possible. The problems were given orally by the experimenter via a two-way intercommunicating system, and the answers were given by the subjects in the same manner.

The problems were of four different 'degrees of difficulty,' ranging from the multiplication of two place by one place numbers to the multiplication of three place by three place numbers. Twelve problems, three of each 'degree of difficulty,' were given to each subject.

Simultaneous samples of the electroencephalograms from the occipital and precentral areas, the electromyogram from the left forearm and the electrocardiogram from lead I were obtained: (1) before any of the problems were given, (2) during the solution of the problems and, (3) following the solution of the problems. There were customarily four 'rest samples,' two before and two following the problems. One sample was taken during each problem, making three 'work samples' for each 'degree of difficulty.'

Ten adult normal subjects (7 females and 3 males) were used in the experiment. Approximately 150 samples each of brain (occipital and precentral), muscle and heart activity were obtained.

Analysis of Data

The various records were read for the average number of waves per second and the amplitude of each sample in the same manner as previously described (9). The reliabilities of the readings were given at that time.

In order to compare the data of the various 'problem periods' and the 'rest periods,' the values of cortical and cardiac frequency and muscle amplitude were expressed in terms of percentage of the respective final 'resting' levels. This standard was used in order to take into account changes which may be due to factors present during the elapsed time of the experiment and which might distort the results if not considered.

Results

1. *Relation of Activity in Brain, Muscle and Heart to the 'Difficulty' of the Problems being Solved.*—Table 1 presents in detail the group

trends in relation to the 'degree of difficulty.' These are graphically presented in Fig. 1 in terms of the percentage of the final 'resting' level.

TABLE 1

ABSOLUTE VALUES * AND PERCENTAGE OF FINAL 'REST' VALUES FOR EACH OF THE MEASURES DURING 'REST' PERIODS AND 'WORK' PERIODS

	Occipital Frequency			Motor Frequency			Muscle Amplitude			Heart Rate		
	Absolute Value		Percentage of Final 'Rest'	Absolute Value		Percentage of Final 'Rest'	Absolute Value		Percentage of Final 'Rest'	Absolute Value		Percentage of Final 'Rest'
	AM	SD		AM	SD		AM	SD		AM	SD	
'Rest$_1$'	12.40	1.03	99	12.56	1.30	106	6.74	3.65	80	75.50	13.83	101
a × de	14.56	2.57	118	13.95	2.19	116	8.65	4.96	103	93.34	13.80	125
ab × de	14.67	2.41	119	13.91	2.55	115	9.30	4.13	110	86.20	16.50	116
ab × def	14.60	2.45	118	14.55	2.00	121	10.28	4.33	122	89.03	14.20	120
abc × def	14.74	2.65	119	14.26	1.97	119	12.82	6.68	152	84.84	14.70	114
'Rest$_2$'	12.50	1.20	100	12.07	1.25	100	8.42	3.00	100	74.50	12.84	100
Average 'Rest'	12.43	1.13		12.33	1.35		7.47	3.53		74.80	13.50	

* Absolute values in terms of waves per second when speaking of cortical frequencies, in terms of microvolts when speaking of muscle amplitude and in terms of beats per minute when speaking of heart rate.

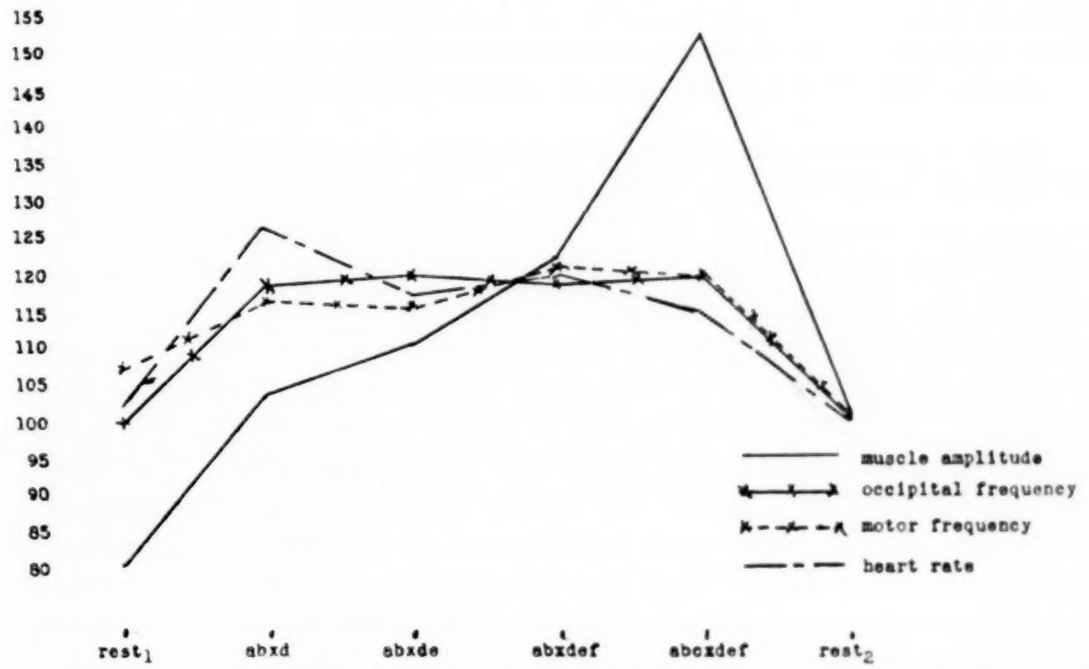

FIG. 1. Levels of activity (in percentage of final 'resting' level) for each of the measures during the 'rest' and 'problem' periods.

It is evident that the muscle amplitude level increased as the 'difficulty' of the problems being solved increased, and that the final 'rest period' level was higher than the first 'rest period.' These findings are in agreement with those of Davis (4).

The frequencies of both brain potentials and heart were faster during problem solving than during 'rest,' but there was very little change in them in relation to the different 'degrees of difficulty' which were presented. The heart rate, in fact, was somewhat faster during the multiplication of two place by one place numbers than it was in any other instance.

TABLE 2

DIFFERENCES AND CRITICAL RATIOS OF THE DIFFERENCES BETWEEN EACH 'WORK' PERIOD AND AVERAGE 'REST'* AND BETWEEN THE VARIOUS 'WORK' PERIODS AND 'REST' PERIODS

	Occipital Frequency		Motor Frequency		Muscle Amplitude		Heart Rate	
	Difference	CR	Difference	CR	Difference	CR	Difference	CR
abc × def—Average 'Rest'	2.31	3.9	1.93	3.9	5.34	3.5	10.04	2.3
ab × def —Average 'Rest'	2.17	4.2	2.22	4.9	2.81	3.2	14.23	4.0
ab × de —Average 'Rest'	2.24	4.5	1.58	2.8	1.83	1.8	11.40	2.6
a × de —Average 'Rest'	2.13	4.3	1.62	3.6	1.18	1.1	18.54	4.8
abc × def—ab × def	.14	.2	−.29	.5	2.54	1.7	−4.99	1.1
abc × def—ab × de	.07	.1	.35	.6	3.52	2.2	−1.36	.3
abc × def—a × de	.18	.2	.31	.5	4.70	2.9	−8.50	1.9
ab × def —ab × de	−.07	.1	.64	1.1	.98	1.0	2.83	.5
ab × def —a × de	.04	.1	.60	1.2	1.63	1.5	4.31	1.1
ab × de —a × de	.11	.2	−.04	.1	.65	.5	7.14	1.6
'$Rest_2$' —'$Rest_1$'	.10	.2	−.49	1.1	1.68	1.4	1.00	.2

* Average 'Rest' refers to the average of the initial and final 'Rest' periods.

Table 2 presents the differences between the various samples and the critical ratios of these differences. The values of 'rest' in this instance are averages of the initial and final 'rest periods.' It is evident that the frequency of the electroencephalogram from the occipital area during problem solving was significantly faster for each group of problems than it was during 'rest.' There were, however, no significant differences in frequency between any of the groups of problems. The same remarks can be made concerning the frequency of the electroencephalogram from the precentral area. The heart rate was significantly faster (or nearly so) during problem solving than it was during 'rest' (the significance of the differences between Group 4 or Group 2 and 'rest' can be questioned). In this instance, also, there were no significant differences between any of the different groups of problems.

When muscle action potential amplitude is considered, only the amplitudes recorded during the solving of three place by two place and three place by three place multiplication problems were significantly higher than those recorded during 'rest.' A nearly significant difference ($CR = 2.9$) was obtained between Group 4 and Group 1, indicating that the more 'difficult' the problem, the higher the action potential level.

The group trends, however, do not adequately depict the course of events when the data of the individual subjects are considered. That is, the predictive values of the above generalizations are low. The following tabulation indicates the number of subjects exhibiting their highest values (of occipital, precentral and heart frequencies, and of muscle amplitudes) in each of the problem series.

	ab × d	ab × de	abc × de	abc × def
Occipital Frequency	1	3	2	4
Precentral Frequency	2	2	4	2
Heart Frequency	5	0	2	1
Muscle Amplitude	2	0	3	5

When individual cases were considered, the tendency for any of the measures to increase as a function of the 'difficulty' of the problems was obvious only in terms of muscle amplitude, *and even there it was step-wise in but three of the ten cases.*

One subject presented lower mean cortical wave per second values in the solution series than in either the initial or the final 'rest periods.' This individual reported that he met with considerable difficulty in the solving of the problems and he became quite emotional during the course of the experiment. Qualitatively, *delta* activity similar to that reporte' by Hoagland, Rubin and Cameron (**10**) was observed on this record.

One subject presented the lowest muscle amplitude value during the solution of two by two place multiplication.

Success and failure have been considered by Davis (**4**) as measures of difficulty. Presumably, the more difficult the problem the greater the percentage of time that it will be failed. Consequently, then, the percentage of problems failed by all of the subjects for each of the 'degrees of difficulty' was computed. These are presented in the following tabulation and indicate that, using failure as a measure of 'difficulty,' there is some empirical justification for considering them as 'degrees of difficulty.'

	Percentage Failed
a × de	23
ab × de	50
ab × def	62
abc × def	87

Upon the same assumption, that failure or success is an index to 'difficulty,' the individuals who had the most 'difficulty' should also fail the greater percentage of the problems. Predicting from the group trends one would thus expect that there would be a relationship between the relative success and failure and relative increases in

muscular activity. In other words, assuming success and failure to be indicative of difficulty and, assuming that increase in muscle action potential is proportional to increase in difficulty, one would expect the individuals with the greatest percentage of failure to have the greatest increases in muscular activity. Table 3, which presents rank

TABLE 3

RANK ORDER CORRELATIONS BETWEEN PERCENTAGE OF FAILURE AND AVERAGE 'WORK LEVEL' OF EEG, EMG AND EKG (IN TERMS OF PERCENTAGE OF FINAL 'REST LEVEL')

	Inferred r	Probable Error
Occipital Frequency and Percentage Failure	.15	.21
Motor Frequency and Percentage Failure	−.16	.20
Muscle Amplitude and Percentage Failure	−.06	.21
Heart Rate and Percentage Failure	−.05	.21

order inferred correlation coefficients between percentage of failure and average work levels, indicates that no such relationship was present between either muscle, heart or brain and percentage of failure.

2. Concomitance of the Variations in Heart, Muscle and Brain.— Table 4 presents the correlations between the measures used in this

TABLE 4

CORRELATIONS * OBTAINED BETWEEN SIMULTANEOUSLY RECORDED SAMPLES OF CORTICAL FREQUENCY, HEART FREQUENCY AND MUSCLE AMPLITUDE (10 SUBJECTS, AVERAGE N = 150)

	Motor Frequency	Heart Rate	Muscle Amplitude
Occipital Frequency	.79 (PE = .020)	.59 (PE = .043)	.23 (PE = .053)
Motor Frequency		.60 (PE = .043)	.17 (PE = .054)
Heart Rate			.30 (PE = .060)

* Rectilinearity was assumed from inspection of scattergrams, however, only the eta between occipital frequency and muscle amplitude is significantly higher than the 'r's.'

study. These correlations are based upon the mean values of the respective variables for each sample secured.

The most interesting figures in this table are the coefficients of correlation for brain frequency and muscle action potential level. These coefficients are not significantly different from the correlations (9) secured during the 'resting state' (difference of Z values is not twice the standard error of the difference (6, p. 207). The correlation between occipital frequency and muscle amplitude when the subjects were not engaged in any specific activity was .17, while during mental

multiplication the obtained correlation was .23. Similarly, during 'rest' the correlation between motor frequency and muscle action potential level was .10, while in the present study it is .17.

All other relationships appear to be significantly higher for 'activity' than those reported for 'rest' (difference of Z values is well above twice the standard error of the difference (**6**, p. 207). The correlation coefficient of .79 between the frequencies obtained from two areas of the cortex during 'work' is significantly higher than the correlation coefficient of .42 secured during 'rest.' The correlations of .59 and .60 between heart rate and brain frequency are definitely higher than the correlations of $-.02$ and $-.03$ obtained during 'rest.' The relationship between heart rate and muscle amplitude obtained in the present study was also higher than that previously obtained (.30 compared with $-.13$).

Discussion

The data presented indicate that both the cortical frequencies and the muscle amplitudes are, on the average, significantly greater during periods of 'mental work' ('mental' multiplication) than during 'rest,' but that there is no significant relationship between these two variables.

Upon the basis of the existing data this particular absence of correlation between central and peripheral phenomena during 'mental work' would not have been predicted. The obvious answer to the failure of adequate prediction is that the technique for measuring central activity –*i.e.*, the electroencephalographic technique) is not valid as far as the problem at hand is concerned. Although this is a possibility which should not be disregarded, certain other propositions should also be considered.

A plausible hypothesis which could account for the lack of correlation would do so in terms of temporal variability in the degree, or level, of the psychological process itself. The solution of a 'one-by two-place' multiplacation problem is more rapid (and hence called 'easier') than the solution of a 'three-by three-place' problem, probably because the elements involved are more readily manipulated. In the problems requiring more time, it is likely that there was great variability from moment to moment in the amount of 'work' being done. If this was the case, it would affect the data markedly, since short samples were taken, and it was not possible to tell whether the subject was working relatively more, or less, during a particular recording. (As a matter of fact, in none of the studies of muscle action potential amplitude in relation to 'work,' has it been known whether the subjects were actually engaged in 'work' *when* the samples were taken.)

This immediately throws open the question of the validity of generalizations from the muscle amplitude data as well as from the cortical frequency data. While muscle tension certainly rises as 'difficulty' increases, it is not known that either the 'difficulty' or the increments in tension are directly related to the work being done *at the moment* that the sample is being recorded.

It should always be kept in mind that group trends apparently do not depict the true picture when individual cases are considered and also that any predictions based upon the group trends apparently do not hold for individual subjects. For example, if increases in muscle action potential are associated with increases in 'difficulty' one would predict that subjects encountering the most 'difficulty' would exhibit the greatest increases. However, using failure as a criterion of 'difficulty,' no relationships were apparent.

Before any specific conclusions can be reached regarding the significance of either increments in muscle tension or increments in cortical frequency, as they may stand in relation to any given psychological process, more adequate controls are essential. Samples secured *between* problems would have aided in the evaluation of the data. No such samples have been recorded in this study, or in any other studies which have dealt with muscle tension. However, Davis (**4**) has 'mixed' his problems so that there was no definite, ordered series from 'easy' to 'difficult' and obtained data indicating that when 'easier' groups of problems followed more 'difficult' groups, the action potential level fell. With reference to electroencephalographic increments, Travis and Egan (**15**), in a study of the effects of verbal stimulation upon changes in alpha frequency, reported a fairly consistent tendency for the frequency to rise during stimulus intervals and to drop during inter-stimulus periods.

With reference to increment in muscular tension, the only data available for comment are those secured during the final 'rest period' (at the very end of an experimental session). Such data indicate that although the final 'rest' level is below the 'work' level, it is *above* the initial 'rest' level. Since this appears to be a substantiated finding (**3**), one might inquire into the diagnostic value of the technique in determining when 'work' begins or ends. In the data presented in this report, the immediate return of the electroencephalographic 'work' frequency to its initial 'rest' frequency is to be noted, as well as the rise of muscle tension from the initial to the final 'rest' period. (This rise is not statistically significant ($CR + 1.4$) but is as great as the rise in Groups 1 and 2.)

The problem of the specificity of the electromyographic technique arises directly from this discussion. A question might be raised as to whether a specific phasic activity is being measured. The answer

to this question has been partially given by Davis (**5**). He finds that certain muscle groups are more likely to be active during certain types of mental activity. For example, the muscular activity was greatest in the right arm during mental multiplication. Unfortunately, the left arm was used in this study, so that direct comparison with Davis' results is not possible. On the basis of his results, however, one would expect a still sharper rise in muscle had the right arm been used. The final answer will come with the recording of activity from a large number of muscle groups under a variety of conditions.

If the heart rate can be considered in any way a measure of general bodily activity, it is evident from the results in this experiment that the brain frequency seems more closely related to that measure during mental multiplication than does the muscle action potential amplitude. Whether this relationship is due to the necessity for an increased blood flow to the cortex or to increased bodily activity during 'mental work' is open to hypothesis alone at the present time, although certain data reported by Lennox and Leonhardt (**13**) indicate that 'mental work' is accompanied by an increase of cerebral blood flow, suggesting that the first possibility may have some experimental support.

Before the electroencephalogram or the electromyogram can be used with any assurance as an index of psychological activity it must be shown that its variability from moment to moment is definitely correlated with variation in psychological process.

Although experimental attack on this problem appears to be quite difficult, investigations of the type employed by Travis (**14**) offer an approach. If 'alpha' frequencies can be demonstrated to be present a greater percentage of the time during the solving of a long problem than they are during the solving of a short problem, and if these frequencies occur during temporary 'lapses of attention' or at definite stages in the process, the data of the present study will be more meaningful.

Summary and Conclusions

1. The purpose of the present investigation was to ascertain the degree of relationship between variations in electroencephalographic frequency, electrocardiographic frequency and electromyographic amplitude during increasingly difficult 'mental multiplications.'

2. Accordingly, simultaneous samples of the electroencephalograms (left occipital and right precentral areas), the electrocardiogram (lead I) and the electromyogram (left forearm) were recorded from 10 adult normal subjects while they were solving problems of varying 'difficulty.'

3. The results indicate a lack of consistent relationship between cortical frequency and muscle amplitude. The muscle amplitude increased with 'difficulty,' while the cortical frequencies, although significantly faster during 'work' than they were during 'rest,' did not show such a step-wise increase. Heart frequency appeared to be closely related to cortical frequency during 'work.'

4. Various hypotheses were offered, not as adequate answers to the disparity of the actual data and the anticipated data, but for rigorous test.

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References

1. Berger, H., Über das Elektrenkephalogramm des Menschens, *Arch. f. Psychiat. u. Nervenkr.*, 1931, **94**, 16–61.
2. Daniel, R. S., The distribution of muscular action potentials during maze learning, *J. exp. Psychol.*, 1939, **24**, 621–629.
3. Davis, R. C., The relation of certain muscle action potentials to 'mental work,' *Indiana Univ. Sci. Ser.*, 1937, No. 5, pp. 29.
4. Davis, R. C., The relation of muscle action potentials to difficulty and frustration, *J. exp. Psychol.*, 1938, **23**, 141–158.
5. Davis, R. C., Patterns of muscular activity during 'mental work' and their constancy, *J. exp. Psychol.*, 1939, **24**, 451–465.
6. Fisher, R. A., *Statistical methods for research workers*, Edinburgh: London, Oliver and Boyd, 1936, Chap. 6.
7. Gibbs, F. A., Interpretation of the electroencephalogram, *J. Psychol.*, 1937, **4**, 365–382.
8. Gibbs, F. A., Davis, H., and Lennox, W., The electroencephalogram in epilepsy and conditions of impaired consciousness, *Arch. Neurol. Psychiat.*, 1935, **34**, 1133–1148.
9. Hadley, J. M., Some relationships between electrical signs of central and peripheral activity: I. During rest, *J. exp. Psychol.*, 1940, **27**, 640–656.
10. Hoagland, H., Cameron, D. E., and Rubin, M. A., Emotion in man as tested by the delta index of the electroencephalogram: I. *J. gen. Psychol.*, 1938, **19**, 227–245.
11. Knott, J. R., Brain potentials during silent and oral reading, *J. gen. Psychol.*, 1938, **18**, 57–62.
12. Knott, J. R., Electroencephalography and physiological psychology: Evaluation and statement of problem (In preparation).
13. Lennox, W., and Leonhardt, E. L., The cerebral circulation: XV. The effect of mental work, *Arch. Neurol. Psychiat.*, 1931, **26**, 725–730.
14. Travis, L. E., Brain potentials and the temporal course of consciousness, *J. exp. Psychol.*, 1937, **21**, 302–309.
15. Travis, L. E., and Egan, J. P., Increase in frequency of the alpha rhythm by verbal stimulation, *J. exp. Psychol.*, 1938, **23**, 384–393.
16. Travis, L. E., and Knott, J. R., Brain potentials from normal speakers and stutterers, *J. Psychol.*, 1937, **2**, 137–150.

THE PERCEPTION OF RELATIONSHIP IN HUMAN ADULTS

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Problem

Köhler (**4**) in 1918 set the conditions for an experiment to determine whether a discriminatory perception of simple forms depends upon the absolute qualities of the figures employed or whether it depends upon the characteristics of a configuration. Investigators before and after Köhler have attacked this problem, using essentially the same techniques, with animals and human infants. Summaries of these investigations have been made by Jackson *et al* (**2**), Tolman (**5**), and Warden and Rowley (**6**).

The essentials of the experiment are as follows: An organism is taught to discriminate between two similar objects which differ along some variable, *i.e.*, size, color, shape, etc. When the organism has demonstrated that it has learned to make the desired discrimination, the 'incorrect' figure is removed, and a third is presented together with the 'correct' figure of the original pair. This third figure is an exaggeration of the quality in which the first two figures differed. Thus, if the subject had learned to select the larger of two squares, the smaller is removed, and the third figure is a larger square than either of the first two.

The first discriminations are considered the learning trials; the presentation of the new stimulus with that stimulus which has been previously learned is considered the critical trial. If, in the example given above, the subject chooses as correct the third stimulus, his response is categorized as 'relational,' on the assumption that the first two stimuli have been perceived as differing in size, and that this relational perception has determined the critical choice. If, on the other hand the subject chooses in the critical trial the same stimulus he had previously learned as correct, his response is categorized as 'absolute,' on the basis that the learning has been specific to the particular figure.

Köhler describes the relational perception as follows:

> The individual colours appearing in a pair attain an inner union. Their role in this union . . . depends not upon their absolute qualities, but upon their places in the system they

compose. If their places with respect to each other are held constant but a variation is made in their absolute quality, the Gestalt and the perceived relationship will be transposed (4, p. 221).

This study was undertaken to investigate this phase of perception in adult human subjects since no previous investigation had used such subjects. Certain other considerations appeared to emphasize the desirability of the present study:

1. Because of the previous use of animals and children, the reasons for the choices made were not apparent. Further light might be thrown on this problem through introspections from adult subjects.

2. Indications were sought as to the comparative results to be obtained through the use of various types of stimuli. The authors questioned the belief expressed by Jackson and Dominguez (1) that the use of stimuli with such 'radical' differences in color as white, gray and black would operate to prevent the 'relative' choice from being made.

3. Since the number of cases reported in most of the experiments is small, the possibility of working with groups was felt worth exploring.

Procedure

The Figures.—The materials used were eight sets of card-boards on which were drawn or mounted various designs. Each set was composed of three figures which were gradations of some variable. The first two figures indicated in each set served for the learning trials. The second and third figures were used for the critical trial, the choice of the third being considered the relational response. Set 1 was composed of three black squares, each of clearly different size, selection of the largest being the relational choice. Set 2 included three squares of equal size, one black, one gray and one white. Set 3 included three playing cards, the 2, 4 and 6 of Hearts. Set 4 presented 3 circles of equal radius but with differing arcs blocked out, the most complete being the relational. Set 5 was composed of equilateral triangles of clearly different sizes, the smallest being relational. Set 6 included squares of equal size of which different amounts of corner had been blocked out, the most incomplete square being relational. Set 7 was again 3 playing cards, the 8, 6, and 4 of Diamonds. Set 8 was 3 squares of equal sizes, one yellow, one orange, and one red.

The sets were constructed in this way in order to assure two trials of each variable. Thus a similar principle was involved in Sets 1 and 5 (size of figure), 2 and 8 (color), 3 and 7 (value of card), and 4 and 6 (extent of completeness of the figure).

This method of presentation permitted each of the four variables to be given in both ascending and descending order. It was felt that such a procedure would exercise additional control over the possibility of choice based upon increase in size or intensity of the stimuli regardless of the experimental situation.

The Groups.—This investigation was conducted as two similar experiments. The first was a group study, the second an individual study. One hundred and seventeen students in the Evening Session at Brooklyn College participated in the group experiment. Of these 74 were men and 43 were women. All were undergraduate students, for the most part in their Sophomore or Junior years. The median age of the group fell between 22 and 23 years.

The 12 men and 8 women who comprised the total of 20 subjects coöperating in the individual experiment varied in age from 21 to 53 years, with the majority between 30 and 36 years of age. All had completed their High School education, the median level attained for the group being between the Junior and Senior years of college.

Experimental Procedure.—For the group experiment, a student was selected from the class, and was given the following instructions for the learning trials:

I am going to present to you two figures. You are to indicate whether the figure on the right or on the left is the correct one. If you are correct, I will say 'correct'; if you are wrong,

I will say 'wrong.' You are given no indication as to what is correct and what is wrong, except that position, right or left, has nothing to do with it.

The class was told to watch what Mr. X did closely. The experimenter then held up the small and medium-sized black squares (Set 1). Mr. X made selections, being told that he was wrong if he selected the smaller square, and right if he selected the larger square. Each time the positions of the squares were changed in a random series. This process was continued until Mr. X had designated the larger square as correct for five consecutive presentations. The small square was then put out of sight, and the medium and large squares were then presented to the class. The class was then told to select what they considered to be the correct figure of the two, and to indicate on a sheet of paper why they made the choice they did. The same procedure was continued throughout the eight sets.

When all eight sets had been presented, the students were asked to answer the following general questions:

1. On what basis did you make your choices?
2. Did you change your basis for making your choices at any time? If so, indicate after which point or points you did so, and why.
3. What did you think was the purpose of the experiments?
4. State any other comments or introspections you wish to report.

The same procedure was used for the individual experiment except that here the individual experienced the learning trials directly. The order of presentation was rotated in an effort to gain some insight into the effect of the position of a set in the series. Thus, the first subject was presented with Set 1 first, the second subject with Set 2 first, etc. The ninth subject was presented with Set 1 first, and the process of rotation continued until the end of the experiment. The experimenter had the advantage of being able to question each subject as to the reasons for each choice as soon as it was made. Pertinent comments were thus obtained which clarified the basis for choice in many instances which would otherwise be ambiguous.

Results

1. Types of Perceptual Response.—Although previous studies dealing with absolute-relational perception have utilized only the two categories in classifying responses, it was found that such a dichotomy in the present study would be misleading. The comments obtained from our subjects revealed that certain choice discriminations did not belong to either the absolute or relational category. If the responses were viewed objectively, as must be done when studying animals, this fact would not have been discovered. It was therefore necessary to introduce a third category.

A few examples may make the point clear. A true relational response was always clear. In such cases subjects explained the reason for their choice as follows: "I chose the one on the left because it is larger, and the larger in the original experiment was correct." Or merely, "Because it is the larger figure." One can substitute such terms as lighter, more complete circle, higher number, etc. to indicate relational response with other figures. The true absolute response is also clear. For example, three squares of different sizes are presented. On the critical trial, the subject selects the medium size square saying that this is correct now because it was correct in the learning trial; he does not view the situation relationally.

Sometimes the subject makes his choice neither relationally nor absolutely, but in terms of another criterion. For example, one sub-

ject's responses, if viewed objectively, would sometimes be called relational, and at other times absolute, but his comments reveal that neither is a true description of his responses. He chose the medium-size square because it was 'appropriate in size, more practical for handling purposes.' He selected the gray square of the white-gray-black series because the "color is not as conspicuous as the other." He picked the most complete circle and most complete square because they were 'more distinct; more complete in their structure.' One of these would otherwise be considered relational, and the other absolute. In the playing card series he selected the card which appealed to him most on the basis of design. Comments of other subjects reveal that these responses were the result of such factors as color preference, utility of the stimuli for practical purposes, position of the colors in the spectrum, aesthetic judgments, etc. We have called this type of response 'Specific.' The meaning and implications of this category will be discussed later.

TABLE 1

FREQUENCY OF TYPES OF PERCEPTUAL RESPONSE IN TERMS OF ACTUAL FIGURE—GROUP STUDY

Figure	*R*	%	*S*	%	*A*	%
1	74	63.2	18	15.4	25	21.4
2	72	61.5	14	12.0	31	26.5
3	77	65.8	12	10.3	28	23.9
4	83	70.9	11	9.4	23	19.7
5	88	75.2	10	8.5	19	16.2
6	87	74.4	11	9.4	19	16.2
7	87	74.4	10	8.5	20	17.1
8	82	70.1	14	12.0	21	17.9
Total	650	69.4	100	10.7	186	19.9

Table 1 presents the frequency of each type of response in each of the critical choice discrimination trials for the group study; Table 2 presents the same material for the individual study. Those responses which were relational appear in the first column *R*; those which were absolute appear in column *A*; and those which were specific appear in column *S*. Together with each frequency is presented the percent which that frequency is of the total number of responses to the particular figure. Table 2 differs from Table 1, however, in that it contains the tabulations of responses in the order in which they were actually presented in rotation form.

More than half of the responses were clearly relational; an average of 69.4 percent for the group study and 56.3 percent for the individual study. The average absolute response was 19.9 percent for the group study and 16.3 percent for the individual study. The average specific response was 10.7 percent for the group study and 27.5 percent for

TABLE 2

FREQUENCY OF TYPES OF PERCEPTUAL RESPONSE IN TERMS OF ORDER OF PRESENTATION-INDIVIDUAL STUDY

Order of Presentation	*R*	%	*S*	%	*A*	%
1	9	45	4	20	8	35
2	11	55	6	30	3	15
3	11	55	6	30	3	15
4	11	55	6	30	3	15
5	11	55	6	30	3	15
6	11	55	7	35	2	10
7	13	65	4	20	3	15
8	13	65	5	25	2	10
Total	90	56.3	44	27.5	26	16.3

the individual study. It will be observed that in the group study there is a larger percentage of relational and absolute responses and a smaller percentage of specific responses than in the individual study. The differences are not statistically significant.

TABLE 3

FREQUENCY OF TYPES OF PERCEPTUAL RESPONSE IN TERMS OF ACTUAL FIGURE—INDIVIDUAL STUDY

Figure	*R*	%	*S*	%	*A*	%
1	11	55	7	35	2	10
2	11	55	6	30	3	15
3	10	50	4	20	6	30
4	12	60	5	25	3	15
5	13	65	5	25	2	10
6	12	60	5	25	3	15
7	10	50	5	25	5	25
8	11	55	7	35	2	10
Total	90	56.3	44	27.5	26	16.3

2. *Influence of the Figures on the Response.*—Table 3 presents the frequency of each type of response in the individual study, reanalyzed in terms of the actual figures used, rather than in terms of the order of presentation. An examination of this table does not reveal any clear influence of the figures on the type of response. This may be due to the fact that for the subjects used, the figures were all very simple, and consequently differences would be negligible.

We find the largest number of absolute choices for sets 3 and 7, which are the playing card figures. The largest number of specific responses, both in the individual and group studies. was to Set 8, color gradations of yellow, orange and red. This result is understandable in view of the fact that some of the introspections revealed

that the relationship was not apparent, and therefore the individuals responded in terms of color preference, or some other criterion. We found in both the group and individual studies that changes in response, particularly changes to specific occurred most frequently at the color series.

3. *The Predominance of the Relational Response.*—We have three sets of facts to indicate that the tendency to make a relational response is more predominant than the tendency to make an absolute or specific response. First, the greater percentage of relational response indicates that this type is the more dominant response.

Second, an examination of Tables 1 and 2 reveals that relational responses tend to increase during the series, and absolute responses tend to decrease. Though these relationships are not statistically significant, their consistency in both studies is noteworthy, especially since it appears from our introspective data presented in Section 4 that these differences are due more to changes in attitude than in the material used.

Table 1 shows an almost steady progression in this direction as we read from the first to the eighth series. The trend is even clearer if the eighth series (the red, yellow and orange cards), which was apparently most ambiguous, is omitted. Table 2 contains evidence of the same nature in the responses of the members of the individual group. An interesting sidelight is the comparison of the trends of the specific responses in the two studies. In the group experiment, there is a regular regression in the frequency of specific responses from the first to the seventh series. At the eighth (color) series there is a reversal. In the individual experiment the color series was presented at different points of the experiment through the process of rotation. It may be because of this fact that we find no trend in the specific responses in this part of the experiment comparable to that of the group study.

In order to analyze this phase of the data further, Table 4 was constructed. This table presents the responses of those subjects in the group study whose mode of response changed during the course of the experiment. Of the 117 subjects who participated, 74 responded consistently throughout the eight series in one manner (56 relational, 10 absolute, and 8 specific). The question arose as to whether analysis of the responses of the 43 subjects whose mode of response varied might not cast further light on the problem of trends.

Examination of Table 4 reveals a clearer trend. The same progression and regressions are evident. Furthermore, if we discount the ambiguous eighth series and compare the frequency of response of these subjects to the first and seventh series, we obtained a critical ratio for the difference which is statistically significant (2.96).

TABLE 4

FREQUENCY OF TYPES OF PERCEPTUAL RESPONSE IN TERMS OF ACTUAL FIGURE FOR SUBJECTS WHOSE RESPONSES VARIED

Figure	*R*	%	*S*	%	*A*	%
1	18	41.9	10	23.3	15	34.9
2	16	37.2	6	14.0	21	48.8
3	21	48.8	4	9.3	18	41.9
4	25	58.1	5	11.6	13	30.2
5	31	72.1	2	4.7	10	23.3
6	31	72.1	3	7.0	9	20.9
7	31	72.1	2	4.7	10	23.3
8	26	60.5	6	14.0	11	25.6
Total	199	57.9	38	11.0	107	31.1

Third, the analysis of individuals' responses indicates the greater tendency towards the relational response. Seventy-four of the 117 cases were consistent in their responses throughout. Of these, the greater number, 56, responded relationally; 10 were absolute and 8 were specific. The remaining 43 subjects showed some change in their responses through the series. Nineteen began either with an absolute or specific response but soon changed to relational. Three began with absolute or relational responses but changed to specific; however the predominant responses of these three individuals were relational. Thirteen showed a mixture of all three responses; but again, the predominant response was relational. Only seven began with relational and changed to absolute with the predominant response absolute, and one began with specific and changed to absolute.

4. Introspective Data.—Inspection of the responses of the subjects in the group study to the four general questions asked at the conclusion of the experiment revealed a sufficient number of clear statements concerning the feelings of the subjects to warrant tabulation and analysis of these statements. The first aspect tabulated was: Did the subject recognize the purpose of the experiment? The second aspect was whether the subject thought he understood the experiment, that is, had a feeling of certainty of response rather than understanding of the purpose. The third aspect was whether the subject recognized that some mode of response other than his own were possible; and the fourth whether the subject felt superior to the experiment.

The subjects were then classified according to their modes of response, and the tabulations were made accordingly. Table 5 presents these data. The first three columns indicate the tabulations for those subjects whose responses were consistently relational, absolute, or specific respectively. Column 4 presents those whose responses were relational at first, then changed to absolute; column 5 those who changed from relational to specific, etc. The last column (*M*) in-

cludes those whose responses varied too much to warrant their classification under any other category. Nine subjects have been omitted from the above tabulations because their reports did not give sufficient information to warrant their inclusion in any of the above groups.

TABLE 5

ANALYSIS OF THE INTROSPECTIVE DATA—GROUP STUDY *

Introspection	*R*		*A*		*S*		*RA*		*RS*		*AR*		*SR*		Multiple		Total	
	N	%	*N*	%	*N*	%	*N*	%	*N*	%	*N*	%	*N*	%	*N*	%	*N*	%
Purpose is consistency†	12	21.2	1	10.0	1	12.5	1	14.3	0	—	1	7.7	0	—	3	27.3	19	17.6
Gained insight	6	11.5	0	—	0	—	0	—	0	—	11	84.6	6	100.0	0	—	23	21.3
Feeling of understanding	43	82.7	9	90.0	2	25.0	7	100.0	1	100.0	2	15.4	0	—	7	63.6	71	65.7
No feeling of understanding	3	5.8	1	10.0	6	75.0	0	—	0	—	0	—	0	—	4	36.4	14	13.0
Does not see alternate possibilities	43	82.7	6	60.0	8	100.0	1	14.3	1	100.0	2	15.4	0	—	7	63.6	68	63.0
Sees alternate possibilities	7	13.5	1	10.0	0	—	0	—	0	—	0	—	0	—	0	—	8	7.4
Sees alternate possibilities later	2	3.8	3	30.0	0	—	6	85.7	0	—	11	84.6	6	100.0	4	36.4	32	29.6
Feels superior to experiment†	9	17.3	0	—	0	—	0	—	1	100.0	0	—	0	—	1	9.1	11	10.2
Total‡	52	48.1	10	9.3	8	7.4	7	6.5	1	.9	13	12.0	6	5.6	11	10.2	108	100.0

* Nine cases were eliminated because of inadequate introspections as follows: 4*R*, 1*RS*, 1*AS*, 1*SA*, and 2 Multiple.

† This category does not add up to 100 percent. The percentage consists of only those who answered this way.

‡ Unlike the other percentages which read vertically, these read horizontally.

a. Purpose of the Experiment.—None of the subjects appears to have recognized the purpose of the experiment. The only answer which was appreciably frequent was that the purpose was to test the consistency of the response.

b. Feeling of Understanding.—On the question of understanding the desired response, 71 stated that they felt that they understood throughout the experiment, 14 reported no understanding at any time during the experiment and 23 reported the gaining of a sudden insight. A feeling of lack of understanding appears to be typical of those who gave the specific response throughout.

The gaining of an insight seems to be most characteristic of those who changed from absolute or specific to relational, 11 out of 13 for the former and 6 out of 6 for the latter. It also occurs in 6 cases of the consistently relational and nowhere else. Indications of insight were usually accompanied by statements of now knowing the purpose of the experiment; that previous statements had been guesses and the

subject now knows the correct answer; or 'suddenly realized a relationship between all three cards.'

c. Awareness of Alternate Possibilities.—Most of those who responded consistently did not indicate an awareness that any response other than the one they gave might be correct (57 out of 70 cases). Twenty-seven of the 38 who changed their responses did indicate an awareness of alternate possibilities as the experiment progressed; the remaining eleven did not. It is interesting that these eleven changed their responses but their reports showed no sign that they realized that they were changing their mode of response, whereas the 27 did.

There were only 8 subjects who saw the alternate possibilities at the beginning of the experiment; of these 7 responded consistently relational and one consistently absolute. Of the 32 who recognized the alternate possibilities during the course of the experiment, 27 were in the groups whose mode of response varied. The remaining 5 maintained their mode of response despite this recognition.

d. Feeling of Superiority.—Eleven subjects gave evidence of feeling 'superior' to the experiment by making such statements as 'childish,' 'juvenile,' 'simple,' "this experiment didn't require much thought." It is interesting that 9 of the 11 were in the consistently relational group.

Discussion

1. The Specific Response.—The introduction of a third category, the Specific, into the problem studied in these experiments is highly important. Its importance rests not only in the manner in which it affects the results of this study; the question which it necessarily raises concerning past experiments with children and with lower organisms cast certain doubts on the interpretation of those data.

Two factors, it appears, operated against the discovery of this third catefory by previous experimenters. The first was an error in logic: it was presumed that the experimental design was sufficiently rigid to enable control of all possible reasons for choice discrimination except the types of responses in which the experimenters were interested. The inextricability of other operative determinants in the materials utilized was overlooked. Thus, although the experimenter may be certain that the presentation of two boxes of differing sizes, but of the same color, wood, perfection of form, etc., will force the discrimination to be based on either relational perception of size or absolute perception of the object, he is prone to forget that he has not divorced the subject from his past experiences with similar or related materials, and that those past experiences may vitiate the validity of his dichotomy. For example, one of our adult subjects selected the largest of the black squares because it was 'easier to look at.'

The second factor which militated against previous discovery of the third category was due to the nature of the subjects used. The inability of children to verbalize properly possible relevant introspections, and the total inability of lower organisms to verbalize, resulted in lack of recognition by the experimenters that any other types of responses might be occurring. There was no reason for them to suspect the limitations of their *a priori* dichotomy.

The existence of this third category necessarily poses the question: may not some of the responses which have been relegated to the categories of 'relational' and 'absolute' in experiments with children and lower organisms really be 'specific'? It would appear that further research is indicated which shall be so devised as to investigate the existence or non-existence in lower organisms of the specific response.

2. Is the Relational Response Fundamental?—Whether the relational or absolute response is the more fundamental is important to our understanding of perception, as well as to the configurational theory expounded by Köhler (4). Koffka elaborates on Köhler's results, and on the difference between adult human perception and that of children and lower organisms as follows:

> His (Köhler's) results indicate that the operation of the 'absolute' factor ceases to be effective with time, and is quickly forgotten. The truly essential, lasting and definite product of learning, he tells us, is dependent upon a configurative function. This statement holds true for all the more primitive forms of life in a measure in which it does not hold for adult human beings. An adult's choice would not have been unhesitatingly in accordance with the structure of the pair, as was the child's. There would have been a question in the adult's mind whether to behave with reference to this configuration, or with reference to the absolute grayness already known to him. Only when we adults are called upon to judge of colours that are qualitatively very like one another—that is, when a small enough interval has been chosen between the negative, positive, and neutral colours—do we likewise fall under the compulsion of a configurative choice. I have frequently verified this result in class-room experiments. Indeed, the difference between the behavior of adult and child shows clearly that the 'absolute' factor is not more primitive but instead a product of higher development. In similar experiments with children, Riekel found that the number of responses to the 'absolute' gray increased with increasing age. It follows that these 'absolute' factors cannot be identical with the 'simple sensations' which, according to the older view, lie at the foundation of all training (3, p. 157).

Koffka indicates above that the absolute response is a higher level response and the relational more fundamental. He further indicates that only where there is a small enough interval between colors will adults display the relational response.

We will discuss two possible definitions of the term 'fundamental.' It can mean (*a*) the behavior of an organism without experience, or (*b*) behavior which is the most readily learned and retained.

Since the subjects that were used in this experiment were obviously organisms with a great deal of experience we cannot discuss our results in terms of the first definition of 'fundamental.' Our results

indicate that the relational response is the most frequent, and that there is a tendency for these responses to increase during the course of the experiment. The subjects who saw alternate possibilities of response were also most frequently relational. However since they see alternate possibilities we can learn very little or nothing about the fundamental nature of the response. Therefore we must turn to the results of those subjects who did not see the alternate possibilities of response. We find when we do so that the most frequent response is still the relational.

Unfortunately, we cannot answer the question of how fundamental the response is in terms of frequency alone. The ability, of the large majority of adult human beings in our culture, to read, for example, would hardly warrant characterization of that form of behavior as fundamental.

If the progressive series of critical choices may be interpreted as a learning process, it would appear that the 'learned' discrimination is more apt to be relational than absolute. Analysis of the data in Table 5 reveals that of 23 subjects who started with absolute critical choice discriminations, 13 changed to relational during the course of the experiment, or a change of 56.5 percent. These figures are to be compared with 8 subjects out of 60 who changed from relational to either absolute (7 subjects) or specific (1 subject), or a change of 13.3 percent. While the relational, from these data, appears to be more frequently learned than the absolute, the data do not lend themselves to any conclusive statement whether either mode of response is or is not fundamental. Frequency in itself is not sufficient evidence.

Nothing in our data yields evidence that is adequate as to whether any of the responses is of a 'higher level.' A phylo-genetic approach, or intelligence test correlation approach, or both would seem to be indicated for the resolution of this problem. Evidence in the literature on perception in lower organisms seems in general to favor the thesis that the relational response is the lower level, or at least that it is the more likely response of the lower organisms (**3**, **4**, **5**, **6**). The evidence is not conclusive, however, and even if it were, we should require experimental verification in adult human beings before the generalization to them could be made.

If we return to the definition of 'fundamental' as the behavior of the organism without experience, then no crucial experiments have been performed. It may be argued that since we are dealing with visual phenomena we can deal with the organism at birth. Even though the organsim has had previous experience before birth, most probably such experience will make very little difference in the perceptions we are discussing.

Where we are examining the perceptions of organisms with ex-

perience, as has been the case in previous experiments, the best way to investigate the fundamental nature of a response is to operate on the second definition of 'fundamental'; *i.e.*, as behavior which is most readily learned and retained. Suggestions are presented below for possible experiments. Undoubtedly the ingenuity of other investigators will reveal other possible fruitful modes of attack on the problem.

Since we know that experience may operate to change the nature of a perception, it is possible that a certain level of the relational response is fundamental, but that the relational response made by our subjects is of a higher level of response. It is possible that for college students the relational response is a form of perception emphasized in their experience. Our introspective data suggest that the subjects did not understand what was wanted in the experimental situation. It is possible that the feeling of insight accompanying a change to the relational response may be the result of cultural influences favoring such a perception. It is also possible, on the other hand, that such a response is actually more fundamental and was evoked for this reason.

The central problem of this experiment was the extension of relational perception from lower organisms to human adults. We feel that the conditions of the experiment were not sufficiently comparable to the situation for lower organisms to make a comparison possible. It is possible that a more comparable situation may be obtained by using more complex material (*e.g.*, verbal) rather than the forms we used in this experiment. Or it may be that a more comparable situation will be produced using the same materials as were used in this study by permitting the subject very little time to make a decision. In such a case it is probable that an adaptation of the reaction time experiment can be profitably made.[1]

While this experiment has not been conclusive in determining whether the relational response is fundamental, it has been useful in several respects: It has indicated the necessity for re-evaluation of previous experiments which have been performed in this area. It has inquired as to the nature of what is fundamental. But the most important contribution is having made it possible to define more specifically what is required in crucial experiments.

3. The Nature of the Stimulus.—Koffka (quotation above) has stated that it is only when a small enough interval between colors has been presented that human adults are forced to react relationally (3). Others (1) have indicated similar conditions in children and lower organisms. The data collected here tend to confirm this thesis in general, although they question the definition of a 'small enough'

[1] The authors are indebted to Dr. Solomon E. Asch for the last suggestion.

interval. Apparently the size of the interval can be larger than has previously been supposed (at least for adult humans), as is demonstrated in the continued dominance of the relational response in the red-orange-yellow series. It is highly probable that the maximum possible size of the interval varies with the organism. Experimental determination of the size possible for any particular organism can very readily be obtained.

Summary and Conclusions

The study was undertaken to extend to the adult human level the absolute-relational choice discrimination experiment which originated with Köhler.

In a group experiment, 117 college students were presented with 8 sets of 3 figures each. These figures consisted of various designs such as squares, playing cards, and incomplete geometrical forms, with progressive gradations of size, color, etc.

The subjects were first taught to discriminate between the first two figures in each set. The critical trial required discrimination between the previously correct figure and the third figure of that set. A control series was run with a group of 20 subjects in an individual experiment.

It was found that:

1. The responses were classified as relational, absolute, and specific. The category 'specific' had to be introduced to classify certain responses which were neither relational nor absolute.

2. The relational response was most frequent, 69.4 percent of all responses; absolute 19.9 percent, and specific 10.7 percent.

3. Rotation of the order in which the figures were presented revealed no clear influence of the figures on the type of response.

4. Seventy-four individuals responded throughout the experiment either consistently relational, absolute or specific; 43 individuals varied from one category to another.

5. For those whose responses varied, the number of relational responses progressively increased during the course of the experiment.

6. Introspections revealed that:

a. None of the subjects recognized the true purpose of the experiment.

b. Most of those who responded either consistently relational or absolute felt that they understood the desired response. Those who responded consistently specific reported no such understanding. Among the subjects whose responses varied, the change to relational response was almost always accompanied by a feeling of having gained a sudden understanding of the desired response.

c. Very few of those who responded consistently indicated an awareness of the possibility of a different correct choice. On the other hand, in almost all cases of varying response there were indications of the awareness of alternate possibilities of correct choice.

7. Certain problems were raised by these data.

a. The discovery of the specific response requires a reëvaluation of previous studies in this area.

b. Though the data in this study reveal the most frequent response to be relational, we have no evidence that this form of perception is fundamental.

c. The size of the interval between figures may be larger than previous research has suggested is favorable to relational response.

8. Experiments which the authors believe will clarify some of the problems raised are suggested.

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References

1. Jackson, T. A., and Dominguez, K., Studies in the transposition of learning by children: II. Relative vs. absolute choice with multi-dimensional stimuli, *J. exper. Psychol.*, 1939, **24**, 630–639.
2. Jackson, T. A., Stonex, E., Lane, E., and Dominguez, K., Studies in the transposition of learning by children: I. Relative vs. absolute response as a function of amount of training, *J. exper. Psychol.*, 1938, **23**, 578–600.
3. Koffka, K., *The growth of the mind*, N. Y.: Harcourt, Brace, 1928.
4. Köhler, W., Simple structural functions in the chimpanzee and in the chicken, In *A source book of Gestalt psychology* prepared by W. D. Ellis, N. Y.: Harcourt, Brace, pp. 217–227.
5. Tolman, E. C., Habit formation and higher mental processes in animals, *Psychol. Bull.*, 1927, **24**, 1–35.
6. Warden, C. J., and Rowley, J., The discrimination of the absolute versus relative brightness in the ring dove, *J. genet. Psychol.*, 1929, **9**, 317–337.

AN INVESTIGATION OF THE ALLEGED FUNCTION OF EMPHASIS IN A SIMPLE DISCRIMINATION PROBLEM [1]

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Several recent investigations have been made to determine the relative efficacy of emphasizing the correct as opposed to the incorrect responses in a simple discrimination problem.[2] These studies were formulated for the purpose of determining whether or not an emphasizing factor exists which might explain the effects of saying 'right' and 'wrong' in terms other than reward and punishment. The results of these investigations appear to be conflicting [3] but the apparent disagreement might possibly be accounted for on the basis of the differences in experimental approaches. For the most part, the procedures have followed one of two methods in presenting the emphasizers. One method consists in presenting the series of discriminations to half of the subjects with an emphasizer accompanying each correct response as a signal that the response is correct and to the other half of the subjects with the emphasizer accompanying each incorrect response as a signal that the response is incorrect. A second method consists in presenting the series of discriminations to all subjects in the same manner with the emphasizer occuring part of the time in connection with the correct response, part of the time in connection with the incorrect response and part of the time not at all.

It has been found, in general, that when the emphasizers are relevant, that is, when they act as definite signals which bear information of correctness, they will result in better performance when they consistently accompany the correct responses than when they consistently accompany the incorrect responses. It has also been demonstrated (Courts, 4) that when the emphasizers are devoid of relevance, that is, when they do not act as signals of correctness, they have little or no more effect upon performance when they accompany correct or incorrect responses than when they do not occur at all.[4]

[1] The writer wishes to express his appreciation to Dr. Robert W. Leeper for his criticisms during the course of this investigation and to the Psychological Laboratory of the University of Oregon for the use of its facilities in conducting this study.

[2] See Courts (4), Courts and Waggoner (5), Gilbert (6), Gurnee (8), Hulin and Katz (9), Muenzinger (12), Muenzinger (13), Porter and Hall (16), Silleck and Lapha (18), Stephens (19), and Tolman, Hall and Bretnall (20).

[3] See Goodenough (7), Hulin and Katz (9), Muenzinger (14), and Tolman (21).

[4] Just what effect the use of shock has as an emphasizer is not clear. Wherever shock has been employed, special explanations have been introduced since shock may have the additional functions of being a potent motivator or a disruptor.

Throughout the various studies where the use of relevant emphasizers resulted in superior performance records when employed in connection with the correct as opposed to the incorrect responses, the obtained critical ratios generally remained below 3.0. Despite the lack of consistently significant differences, some investigators have interpreted the results as offering new evidence on the nature of learning with special reference to the law of effect. Although it is often overlooked, it is obvious that facts of learning must be inferred from performance records. Further, there is always the possibility that factors may arise which influence the records so as to render them subject to misleading interpretation. None of the previous experimenters has demonstrated conclusively that the differences between performance records of subjects whose correct responses were emphasized and subjects whose incorrect responses were emphasized were true differences in amount learned and not artifacts resulting from the different conditions under which the subjects were forced to express what they had learned. It was for the purpose of examining this possibility that the present investigation was formulated.

The general procedure consisted in having matched groups learn series of paired numbers, each group under different conditions of employment of emphasizers. After the learning period was concluded, the numbers were again presented but were no longer paired. It was assumed that if the superior performance of a group during the learning period was truly indicative of superior learning, such a group would also be superior in identifying the numbers as having been correct or incorrect. Conversely, it was assumed that if no group performed significantly better in identifying the numbers under the new conditions, the original performance differences in the learning situations would not be true differences in amount learned.

Experimental Apparatus and Procedure

The apparatus, a modification of that reported by Porter and Hall (16), was so constructed that it served to present to the subject a pair of numbers in such a way that either one of the pair could be accompanied by the emphasizer regardless of whether the subject made a correct or an incorrect choice. One emphasizer consisted of a red light directed on one or the other of the numbers. The second emphasizer consisted of a white light originating from one of two pilot-lights attached on the front of the apparatus and to each side of the exposure aperture. Two telegraph keys attached on the front of the apparatus served as means by which the subject might indicate his choice of either the left-hand or right-hand number in the pair.

In order to obtain matched groups each subject was required to practice upon a series of 12 pairs of 3-place numbers. The serial order of presentation was changed with each trial and the location of the correct number irregularly alternated from the left to the right. The subject was given instructions to the effect that after each choice the experimenter would announce whether the choice had been correct or incorrect. A limited period was provided in which to make a choice and a brief interval allowed following the announcement for studying the numbers. The practice continued for 10 consecutive trials regardless of the excellence of performance. On the basis of the score made on this series the subject was assigned to one of three experimental groups.

The learning material used in the experimental period consisted of 2 series of 15 pairs of 2-place numbers. These were so rotated that each combination of a series and an emphasizer occurred an equal number of times as the first and second series to be learned. As in the matching series, different serial orders of presentation were employed and the location of the correct number was irregularly alternated in position. During this period no announcements of correctness were made. The subjects gained this information from the position of the emphasizer in accordance with the directions given under the experimental conditions. The emphasizer operated during the brief interval immediately following a choice. Practice for 8 consecutive trials on each series regardless of excellence of performance constituted the experimental learning period.

The subjects of one group were instructed always to press the key which corresponded in position to the correct number and that with each choice the emphasizer would accompany the correct number. This group is hereafter referred to as the Press Right-Emphasis Right (*PR–ER*) group. The subjects of a second group were instructed always to press the key which corresponded in position to the correct number but that with every choice the emphasizer would accompany the wrong number. This group is called the Press Right-Emphasis Wrong (*PR–EW*) group. The subjects of the third group were instructed always to press the key corresponding to the wrong number but that the emphasizer would always accompany the correct number. This is the Press Wrong-Emphasis Right (*PW–ER*) group. Each group contained 28 volunteer subjects from elementary psychology classes.

At the conclusion of the learning period, each subject was asked to describe just what he had done in learning the two series. Subsequently, the subject was taken into another room and given the post-test, a sheet of paper on which appeared all of the 60 numbers that had been used in the experimental series and in addition 30 numbers which had not been used at all. These numbers were no longer paired but were arranged on the sheet in ascending order of value. The subject was asked to mark each number as having been correct, incorrect or not-included and to guess where there was reasonable certainty of being correct.

Data and Results

The mean errors and the standard deviation of the mean for each of the three groups on the matching series are as follows: *PR–ER*, 39.32 ± 2.07; *PR–EW*, 38.43 ± 1.65; *PW–ER*, 34.79 ± 1.88. Intergroup comparisons of these differences indicate no one group to have achieved significantly more as evidenced by critical ratios of 1.6 and below.

During the experimental learning period, however, definite differences in performance were evidenced. These differences and their significance are presented in Table I. The data are presented for the

TABLE I

Group Differences in Errors on Experimental Learning Series

Groups	1st Series			2nd Series		
	M	σ_M	*CR*	*M*	σ_M	*CR*
PR–ER *PR–EW*	17.86 23.57	1.50 1.88	5.5	15.43 19.25	1.88 1.26	4.2
PR–ER *PW–ER*	17.86 24.86	1.50 2.11	6.2	15.43 21.50	1.88 2.16	3.6
PR–EW *PW–ER*	23.57 24.86	1.88 2.11	1.1	19.25 21.50	1.26 2.16	1.7

first and second series learned and represent the performance for trials four through eight. The initial trials were not included because of the heavy influence of chance factors.

The *PR–ER* group is seen to be significantly superior to the other two groups, the differences between the latter two being insignificant.

Coefficients of reliability obtained by correlating the scores on the first series learned with the scores on the second series in each of the experimental conditions were as follows: *PR–ER*, .72 ± .09; *PR–EW*, .76 ± .08; *PW–ER*, .65 ± .10. These coefficients demonstrate no marked differences in reliability of the learning series from one experimental condition to the next.

The data from the post-test are presented in Table II in terms of

TABLE II

PERCENTAGE CORRECT IDENTIFICATIONS ON POST-TEST

Group	Correct	Incorrect	Not-Included	Total
PR–ER.....	68.7	47.1	63.7	60.3
PR–EW.....	62.1	39.1	77.1	59.2
PW–ER.....	64.2	39.9	70.7	58.5

the percentage of correct, incorrect, and not-included numbers which were correctly identified as having been correct, incorrect, and not-included, respectively. The base used was the total number marked in some way and not the total number of items presented.[5]

Intergroup comparisons of the performances on the post-test showed no one group to be reliably superior at any point. None of the critical ratios exceeded a value of 1.0.

INTERPRETATION OF RESULTS

The results show that three rather closely matched groups turned in quantitatively different performance records when each learned

[5] This was necessary since the subjects did not all mark the same number of items. Since this was considered a decided weakness at the most crucial point, a repetition of the experiment was made under conditions slightly modified for group testing rather than individual testing. The subjects were directed to mark all of the items on the post-test. Only the conditions of 'emphasis on right' and 'emphasis on wrong' were used. The results were in general agreement with those presented in the body of this paper. In addition to percentage comparisons on the post-test, rank order correlations were determined for both groups on the number of errors on the last six trials and the number of errors on the post-test. For the 'emphasis on right' group the value of rho was .83 ± .03 indicating the probable measurement of substantially the same factors in the learning period and in the post-test. For the 'emphasis on wrong' group a low negative value of rho, −.11 ± .07, indicated the probable measurement of different factors in the two performance periods. This implies that the learning period performance records of the 'emphasis on wrong' group does not measure necessarily the same thing as the learning period performance records of the 'emphasis on right' group and therefore the records for the two groups in the learning period are not strictly comparable.

under different conditions in employment of emphasizers. The fewest errors were made by those subjects who learned to select the correct number when the correct number was always accompanied by the emphasizer. Subjects who learned to select the correct number when the emphasizer always accompanied the wrong number performed significantly poorer.[6] Subsequently, however, the instructions and conditions were made uniform from group to group when each subject was asked to identify the no longer paired numbers. Under these new conditions no one group performed in a manner significantly superior to that of any other group. The fact that the *PR–EW* group did not show an inferior performance compared with the *PR–ER* group is interpreted to mean that *it had not learned significantly less in the learning period and that other factors must be called upon to account for the inferior learning period performance record.*

Such an interpretation implies that performance records do not of necessity represent rate of acquisition nor amount learned. That such an implication is justifiable is evident from the work of many investigators. Blodgett (**2**), Tolman and Honzik (**22**), and others have demonstrated that the performance curves of rats are greatly affected by delayed introduction of reward and by removal of reward while the learning remains relatively unaffected. Similar effects have been demonstrated with human subjects (Porter, **15**), under conditions of delayed instructions to learn. Another line of evidence arises from the facts of overlearning. Continued practice beyond the criterion of a set number of errorless trials results in additional learning which does not evidence itself in the original performance records. It has also been suggested by Bryan and Harter (**3**), Batson (**1**) and others that the plateaux which often appear in 'learning' curves are due to inadequacy of measurement of the learning which may be occurring.

In attempting to account for the inferior performance of the *PR–EW* group during the learning period two sources of possible influence suggest themselves. The first source is that of errors which might arise as a result of responding to what may be termed 'subliminal cues.' It is possible that during the initial stages of learning, before the subject is able to verbalize that "this number is correct" and "that number is wrong," he learns that something happens in

[6] All of the subjects but one in the *PW–ER* group reported at the end of the experimental learning session that they had changed the instructions given them in such a way that the numbers designated by the instructions as 'correct' were treated as if they were 'wrong.' According to the interpretation placed on the emphasizer, then, this group received the emphasis in the same manner as did the *PR–EW* group. To all intents and purposes it appears that these two groups might be considered to have learned under identical conditions and therefore to be comparable. Although they are meant to be so treated in this paper, complete disregard of the *PW–ER* group results would in no way affect the conclusions reached.

connection with the one number and not the other As several of the subjects expressed it, "one number stands out," although that number could not be identified as 'right' or 'wrong.' For the *PR–ER* group the conditions of being correct and being emphasized always accompany the number to be learned. For the *PR–EW* and the *PW–ER* groups, however, the condition of being emphasized accompanies one of the numbers while the condition of being correct accompanies the other of the pair. If, as suggested by the reports of the subjects, a number is rendered more perceptually distinct by being accompanied by the emphasizer, it is possible that the subjects will, in the absence of definite knowledge of correctness, respond to the number more perceptually distinct on the chance, as it were, of being correct. If this happened to at least some extent, then the subjects who received the emphasis on the correct number were credited for responding to the same cues for which the subjects who received the emphasis on the wrong number were discredited.

The second source of possible influence is that of errors which might arise as a result of a conflict between conditions of performance imposed by the instructions and previously established habits. The subjects of the *PR–ER* group were instructed to respond to the locus in which the emphasizer occurred. The subjects of the *PR–EW* and *PW–ER* groups were instructed to respond opposite to the locus in which the emphasizer occurred. The latter situation is much like asking the subject to look toward the right and point to the left or to nod his head and say, 'No.' Many of the subjects of the *PR–EW* and *PW–ER* groups reported during the learning period that they had pressed the wrong key even though they knew it to be the wrong key. One individual actually reported sixteen times having made this confused response. Another individual reported at least once during each of the last five trials having pressed the key he knew to be incorrect. In only two cases did the subjects of the *PR–ER* group report having made such a confusion in response. To what extent the performance records of the groups differed as a result of this type of error is not known as no satisfactory method of measurement was available. However, the more nearly equal performance of the groups on the second series learned suggests that with continued practice the groups might eventually perform equally well in the learning period.

To recapitulate, the *PR–EW* group subjects seem subject to errors during the learning period due to factors other than amount learned. This possibility appears to be strengthened by the low correlation between learning period performance and post-test performance for *EW* group as compared with the high correlation between the records of the same periods for the *ER* group found in a

supplementary study discussed in footnote 5. That continued practice might eventually result in marked reduction of the differences in performance records for the groups is suggested by the drop in magnitude of differences on the second series learned. It appears justifiable, then, to view the learning period performance records of the *ER–EW* group as being unsuitable to use as measures of the amount the group learned.

In terms of general learning theory, the present results suggest the need for the reinvestigation of previous studies of similar nature which indicate the operation of an emphasis factor as a decisive determinant in simple discrimination problems. There is need to determine whether the performance differences were not due to artifacts arising from the different conditions under which the subjects were forced to express what they had learned.

Moreover, the results may be said to disagree with the formulation of Lorge (**11**), Tuckman (**23**), Rock (**17**) and others to the effect that the occurrence of a wrong response results in measurably harmful effects on learning. While the subjects who received emphasis in connection with the incorrect responses performed significantly poorer than did subjects who received emphasis in connection with the correct responses, this difference in performance existed only during the learning period when, as has been pointed out, the conditions for expression of what had been learned were different for each group. Yet later in the post-test situation when the conditions for expression had been made uniform from group to group, no significant differences between the performances of the subjects who had learned under different conditions were found despite the fact that the subjects whose incorrect responses had been emphasized had made significantly more incorrect responses during the learning period.

Summary and Conclusions

Matched groups learned series of number pairs. The subjects whose correct responses were accompanied by an emphasizer performed significantly better than subjects whose incorrect responses were accompanied by an emphasizer. Subsequent to the learning period, the subjects were presented with a sheet of paper bearing all of the numbers, no longer paired, which had appeared in the learning series and in addition some numbers which had not appeared. No one group identified significantly more of the numbers as having been correct, incorrect or not-included than did any other group. From this it was concluded that the group which had performed significantly poorer in the learning situation had not learned significantly less and that the inferior performance was due to factors other than the amount learned.

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References

1. Batson, W. H., Acquisition of skill, *Psychol. Monog.*, 1916, **31**, No. 3.
2. Blodgett, H. C., The effect of the introduction of reward upon the maze performance of rats, *Univ. Calif. Pub. Psychol.*, 1929, **2**, 113–134.
3. Bryan, W. L., and Harter, N., Studies in the physiology and psychology of the telegraphic language, *Psychol. Rev.*, 1897, **4**, 27–53.
4. Courts, F. A., *The alleged retroactive effect of visual stimuli subsequent to a given response*, 1935, Unpublished Master's thesis, University of Oregon Library.
5. Courts, F. A., and Waggoner, D., The effect of 'something happening' after a response, *J. exper. Psychol.*, 1938, **22**, 4, 383–387.
6. Gilbert, R. W., A further study of the effect of non-informative shock upon learning, *J. exper. Psychol.*, 1937, **20**, 396–407.
7. Goodenough, F. L., A note on Tolman's 'disproof' of Thorndike's law of effect, *J. exper. Psychol.*, 1933, **16**, 459–462.
8. Gurnee, H., The effect of electric shock for right responses on maze learning in human subjects, *J. exper. Psychol.*, 1938, **22**, 354–364.
9. Hulin, W., and Katz, D., A comparison of emphasis upon right and upon wrong responses in learning, *J. exper. Psychol.*, 1935, **18**, 638–642.
10. Hulin, W., and Katz, D., A comparison of the bell signal for right and wrong responses; a reply, *J. exper. Psychol.*, 1936, **19**, 117–119.
11. Lorge, I., The efficacy of intensified reward and of intensified punishment, *J. exper. Psychol.*, 1933, **16**, 177–208.
12. Muenzinger, K. F., Motivation in learning. I. Electric shock for correct responses in the visual discrimination habit, *J. Comp. Psychol.*, 1934, **17**, 267–277.
13. Muenzinger, K. F., Motivation for learning. II. The function of electric shock for right and wrong responses in human subjects, *J. exper. Psychol.*, 1934, **17**, 439–448.
14. Muenzinger, K. F., A note on the relative efficacy of a bell signal for right and wrong responses in maze learning, *J. exper. Psychol.*, 1936, **19**, 116–117.
15. Porter, E. H., Jr., The influence of delayed instructions to learn upon human performance, *J. exper. Psychol.*, 1938, **23**, 633–640.
16. Porter, E. H., Jr., and Hall, C. S., A further investigation of the role of emphasis in learning, *J. exper. Psychol.*, 1938, **22**, 377–382.
17. Rock, R. T., The influence upon learning of quantitative variation of after effects, New York: Bureau of Publications, Teachers College, Columbia University, 1935.
18. Silleck, S. B., Jr., and Lapha, C. W., The relative effectiveness of emphasis upon right and wrong responses, *J. exper. Psychol.*, 1937, **20**, 195–201.
19. Stephens, J. M., The influence of punishment on learning, J. *exper. Psychol.*, 1934, **17**, 536–555.
20. Tolman, E. C., Hall, C. S., and Bretnall, E. P., A disproof of the law of effect and a substitution of the laws of emphasis, motivation and disruption, *J. exper. Psychol.*, 1932, **15**, 601–614.
21. Tolman, E. C., The law of effect: a reply to Dr. Goodenough, *J. exper. Psychol.*, 1933, **16**, 463–470.
22. Tolman, E. C., and Honzik, C. H., Introduction and removal of reward, and maze performance in rats, *Univ. Calif. Pub. Psychol.*, 1930, **4**, 257–275.
23. Tuckman, J., The influence of varying amounts of punishment on mental connections, *Teach. Coll. Contrib. Educ.*, 1933, No. **590**, pp. v + 45.

WORDS AS CONFIGURATIONS

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If, as Gestalt advocates contend, configurational tendencies of stimulus patterns are enhanced by meaningfulness, good evidence of this should be found in the words we use. According to this hypothesis, it should be more difficult to rearrange the letters of a given word into a new word that it is to do the same with a pattern of jumbled letters.

Hollingworth (**2, 3**) performed experiments calling for the solution of both meaningful and meaningless patterns of letters. College students, and children between eight and ten years of age were used as subjects. With both groups he concluded that meaningful words did not resist rearrangement any more than did meaningless groupings of letters. On the other hand, Devnich (**1**) set up a similar experiment which produced statistically reliable differences which indicated the operation of words as Gestalten. She concluded: "Configured forms convey meaning and resist being changed into other words with greater tenacity than do meaningless arrangements."

The present writers feel that some of the techniques used by both the above investigators are questionable. First, a definite class of words was used, such as animals, psychological terms, names of cities. This is bound to introduce a differential knowledge factor and also causes subjects to search their memories for names which will fit the letters rather than to focus attention upon the stimulus pattern. Second, many of the configurations used were deficient in meaning. That is, the combinations were sometimes not actual words or were in phonetic spelling, or the two small words used often had no obvious relationship. For example: FOAL FUB (buffalo), TRUBY FELT (butterfly), RAZ LID (lizard) or LEAP DOT (tadpole). Third, use of the group method precluded study of the individual approach and other possible individual differences. Finally, presentation of a long list of words with a time limit for solution made the selection of words to be solved too dependent on chance factors, and may have involved distractional features.

An attempt was made to correct these objectionable procedures in the first section of the present experiment. Several hundred common English words were chosen, ranging from five to seven letters in length, the letters of which could be formed into two smaller words. Of these, the apparently meaningful configurations were chosen and

given to a group of people for the purpose of weeding out all words with two or more solutions. From those still remaining the twenty best configurations were finally selected.

Then a set of meaningless arrangements was made, which was equated to the configured list as to the number of letters and vowels and consonants in the words. These presumably equated lists were presented, one word at a time, to each of twenty college students, and time noted for each solution. Results indicated that the meaningful combinations resisted rearrangement to a greater degree than the meaningless forms.

However, there was no definite proof that the two above lists were exactly equated. For this reason, a second approach was made, which, like the experiments of Hollingworth and Devnich, presented the words in counterbalanced lists to groups of subjects. In this instance the meaningful combinations were used as in the first section, but the meaningless forms were divided into consonants and vowels so as to be as free from meaning as possible. Examples are:

Group I	Group II	
NRD OU	DO RUN	(round)
USE LAX	XSL AEU	(sexual)
LSMB YO	MY SLOB	(symbol)
SEE MOON	MSN OEEO	(someone)

Each group of about fifty students was given one of these lists of twenty words to solve. Results showed, as in Hollingworth's experiment, that there were no significant differences in the difficulty encountered when working on meaningful as compared with meaningless words. No evidence was found which would support the general hypothesis that the letters of meaningful words are harder to manipulate and rearrange than those of a meaningless pattern.

It seemed possible, however, that use of the group method might hide interesting individual differences. Sargent (4) recently reported on a variety of individual differences, qualitative as well as quantitative, which appear in the solving of disarranged word problems.

Using the materials from the above section, two lists of nine words each were chosen (one meaningful, one meaningless) which were equated for difficulty. The total number of solutions per list was the same as shown by the group method. Each list, furthermore, had virtually the same number of letters, the same distribution of letters per word and the same number of vowels and consonants. The two lists were as shown in Table I.

The subjects for this part of the experiment were sixteen college students selected at random. Each subject was presented, first of all, with a training series of ten scrambled words, each printed on a small card, and was instructed to solve the problems orally by talking out

his reactions as he worked. After finishing the training series and thus learning to talk out their reactions, the subjects began the experimental series, in which the meaningful and meaningless combinations were presented in a random order which was constant for each

TABLE I

(Number preceding word indicates order of presentation) [1]

Meaningful	Meaningless
1. DO RUN	2. PCN OTO
4. MY SLOB	3. XSL AEU
8. TINY AXE	5. RMN OI
9. NO ROD	6. LPM YI
10. HOT TAR	7. MSN OEEO
12. REAL RUG	11. DLRC EA
13. REAL NUT	14. GSMLP EI
16. TIN NOSE	15. DGNB AEO
17. GLUE GAG	18. LRDM EAE

[1] Solutions are as follows:

1. Round	4. Symbol	7. Someone	10. Throat	13. Neutral	16. Tension
2. Canopy	5. Minor	8. Anxiety	11. Cradle	14. Glimpse	17. Luggage
3. Sexual	6. Imply	9. Donor	12. Regular	15. Bondage	18. Emerald

subject. (See Table I.) As a subject turned over a card and began to work, the experimenter kept time with a stopwatch and made a record of all spoken material. When he finished the entire list the subject was questioned as to his general reactions to the material. No cues were given as to the purpose of the experiment, so a fairly unbiased report could be expected.

Quantitative results revealed that ten of the sixteen subjects showed differences, in which the total time taken to solve meaningful words was 15 percent or more longer than for the meaningless list, with a median of 30 percent. Four subjects had differences ranging from 1 to 5 percent longer for the meaningful list. Two subjects had differences in the other direction of 43 and 85 percent respectively.

Qualitative data obtained from the retrospective and talking-out records of the first ten subjects mentioned gave evidence of the operation of, or at least a consciousness of, greater resistance to rearrangement on the part of the meaningful configurations. For example:

Subject J. A., talking-out while solving HOT TAR:

> Hot tar—tar oat—or, ort, art, art, hart, hotter, hot art; H-O, H-O-T, no! Otter, hatro, tar, tar, tar, hatter, no! I can't seem to concentrate on anything else but *hot tar*.

Subject V. S., retrospective report:

> Those words that made sense were harder to get out of my mind; they kept on bothering me and sort of wouldn't let me break them up.

Subject P. Z., retrospective report:

> Say, those words that made sense were harder.

Of the four subjects showing no significant differences in time taken to solve meaningful and meaningless words, three gave evidence of a mental set which may have functioned in such a way as to destroy the configurational effect. For example:

Subject F. S., retrospective report:

> I felt that the ones that made sense were that way purposely to confuse me so I didn't pay attention to them but concentrated on the separate letters.

Subject E. S., retrospective report:

> The meaningful combinations seemed more difficult and therefore I decided they were the key words and did my best to concentrate on letters and not words.

Of the remaining two subjects with differences in the opposite direction, one showed no recognition of the difference between the two kinds of presentation, and the other claimed an advantage in the configured form. The retrospective report of the latter subject, A. B., is interesting:

> It seems I do better when they are already made up into words. When they are divided into two words, you can be practically sure those words won't be used. This seems to enter into my procedure.

Study of the talking-out data showed no relationship between the subject's method of procedure as such, and the presence or absence of difficulty in dealing with the meaningful combinations. Typical procedure for all subjects was to read over and pronounce the letters or syllables during the first several seconds, then, if the solution did not emerge, to begin a trial and error search for initials, prefixes and suffixes until the solution arrived.

In conclusion:

First, no evidence is found for a general statement that a meaningful word is a more cohesive configuration than a meaningless pattern of letters. Second, many individual subjects do, on the other hand, experience greater difficulty in rearranging the letters of meaningful words. (The large percentage of the sixteen subjects who manifested this interference effect is not, of course, indicative of the proportion of subjects who might react the same way in another group.) Third, the mental set or attitude of the individual subject is a crucial factor. Awareness of the problem plus a conscious effort to ignore the meaningful configuration may diminish or even overcome interference effects.

In the opinion of the authors these results indicate the importance

of quantitative and qualitative study of the behavior of each individual in experiments on problem solving.

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References

1. Devnich, G. E., Words as gestalten, *J. exp. Psychol.*, 1937, **20**, 297–300.
2. Hollingworth, H. L., The conditions of verbal configuration, *J. exp. Psychol.*, 1935, **18**, 299–306.
3. Hollingworth, H. L., Verbal gestalt experiments with children, *J. exp. Psychol.*, 1938, **23**, 90–95.
4. Sargent, S. S., Thinking processes at various levels of difficulty, *Arch. Psychol.*, 1940, **249**.

MEASUREMENT OF A SUBJECTIVE ASPECT OF LEARNING

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A complete description of learning would include a record of the subjective changes which accompany the well-known changes in overt performance. Increases in speed and accuracy are easily recorded; decrease in metabolic cost may be recorded with suitable apparatus (3). Changes in subjective aspects of the learning process have frequently been pointed out—even in elementary textbooks, *e. g.*, Ruch (4)—but quantitative records have not, to the writers' knowledge, been published. This communication describes a method and presents a learning curve for one subject.

In the usual learning experiment with human subjects—and perhaps outside the laboratory—the material is recalled more or less accurately and expressed in the form of a judgment with more or less confidence. Increase in confidence in recall is characteristic of the learning process, and is implied in the word 'mastery.' James Mill (2, p. 468) mentioned certainty as one criterion of the strength of an association. We can, therefore, ask the subject to construct a subjective scale of confidence, and to use it consistently. Thus we can record changes in quantitative form as learning progresses.

Confidence was expressed in terms of a scale ranging from 0 to 100, wherein 0 meant 0 percent confidence or a pure guess, 1.00 meant 100 percent confidence or complete certainty, and the intermediary points were defined in reference to the end points. A little preliminary practice in the use of such a scale was given. It has previously been shown that such a scale can be used consistently in psychophysical experimentation and shows a definite functional relation—in the form of a normal ogive—to the stimulus variable (1).

The subject was a woman of middle age. Twenty-five photographs of people were presented, and the subject was to associate with each picture: (*a*) a name, (*b*) a vocation, and (*c*) a recreation. Thus there were 75 interesting items to be learned. Motivation was quite stable. Ten trials were given, one day apart. Knowledge of results was not given during the trials but, at the end of each trial, the subject was permitted to go over the material, noting errors and memorizing.

Items were supplied for matching with each picture so the judgments were made in true-false form. Confidence was reported after

each of the 75 judgments in each trial. Time of response was taken by a stop-watch. Thus learning curves can be graphed from three sets of data: (1) number of successes (graphed in percentage units for comparison with the confidence curve), (2) median time of judgment, and (3) mean confidence.

In Fig. 1 the curves for accuracy and for time are of the usual form. The confidence curve parallels the accuracy curve after the second trial and shows similar fluctuations. Near the beginning of the confidence curve there is an interesting inversion. The subject expressed moderate confidence even on the first trial although one would logically expect pure guessing. Then, when the difficulty of the task was appreciated, confidence went down, to rise later on a firmer basis. This is the experimental equivalent of a sequence which is occasionally observed outside the laboratory.

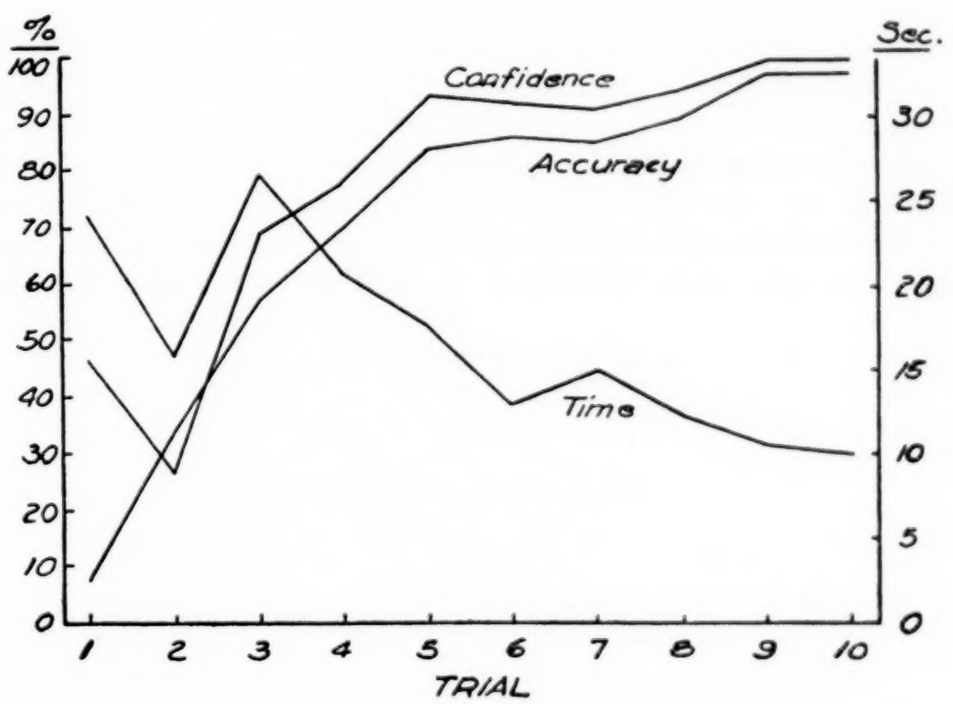

FIG. 1. Learning curves for accuracy, confidence and time.

Figure 1 is offered as rounding out the quantitative description of the learning process. Curves for other subjects and for other material would probably differ in details. The confidence curve might be below the accuracy curve, or farther above. When the confidence curve does not parallel the accuracy curve, one would suspect a change of set, as at the beginning of the experiment reported here. It would be interesting also to determine whether the curves of other subjective aspects of the learning process, *e. g.*, the feeling of facility, would parallel, or coincide with, the confidence curve. And it is suggested

that the feelings of a human subject at the 'choice point' should be considered in an adequate theory of learning.

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1. Johnson, D. M., Confidence and speed in the two-category judgment, *Arch. Psychol.*, 1939, **241**, pp. 52.
2. Mill, James, In Rand: *The Classical Psychologists*, Houghton-Mifflin, 1912.
3. Rounds, G. H., Schubert, H. J. P., and Poffenberger, A. T., Effects of practice upon the metabolic cost of mental work, *J. gen. Psychol.*, 1932, **7**, 65–79.
4. Ruch, F. L., *Psychology and Life*, Scott Foresman, 1937.